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THE DESIGNATION OF COMBINATION TONES

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Nearly all investigators who have dealt with the combination tones have referred to them as belonging to various orders. No agreement may be found, however, as to how these orders are defined. Many have used ordinal terms in a vague manner, without any mention of a system, while others have employed a particular type of designation without clear indication of its basis.

Four principal methods of designation may be distinguished. They are (1) the original system proposed by Hällström, (2) Krueger's modification of Hällström's procedure, (3) a method roughly outlined by Stumpf, and (4) a system implicit in Helmholtz's distortion theory.

1. *Hällström's system.*—The problem of the orders of combination tones began with Hällström (1). Earlier, for nearly a century after Tartini's discovery of the 'third' sounds about 1714 (4), only the one type was known; it was called 'Tartini's tone,' or the 'grave harmonic.' In 1800, Young (15) reported the discovery of another combination tone, which he called a 'secondary sound' in apposition to Tartini's. Other combination tones were then observed, possibly by Scheibler and certainly by Hällström, and the latter appreciated the need for a theory of their origin.

The theory that then was current was the beat-tone theory, said to have been suggested by Romieu, and definitely formulated by Lagrange (6) and Young. According to this theory, the new component was simply a result of the fusion of rapid beats. This view followed naturally from the observation

that the frequency of the one combination tone then known, now called the first difference tone, is exactly equal to the beats of the primaries.

The discovery of other components, of different frequencies, produced a difficulty for the theory. It is curious that Young, who added the 'secondary' tone, failed to take note of the complication that it entailed. Hällström, who reported four of five combination tones in all, sought to extend the beat-tone theory to account for them.

Hällström's suggestion was that beat-tones arise not only from the primaries directly, but also in interactions between one of the primaries and a combination tone already formed or between two combination tones. The number of interactions determines the order of the combination tone.

The operation of the principle is best shown symbolically. Let h and l represent the higher and lower of two primary tones, whose frequencies are, let us say, 1300~ and 1000~ respectively. The first four orders of combination tones then are as follows:

$D_1 = (h - l).$	$(1300 - 1000 = 300).$
$D_2 = (l - D_1) = (2l - h).$	$(1000 - 300 = 700).$
$D_3 = (h - D_2) = (2h - 2l).$	$(1300 - 700 = 600).$
$D_4 = (D_2 - D_1) = (3l - 2h).$	$(700 - 300 = 400).$

The combination tones observed by Hällström were all difference tones in present terminology; hence the use of the designations D_1, D_2, etc. It was several years later that Helmholtz (**2**) discovered the summation tones, and thereupon distinguished these and the difference tones as two classes under the older designation of combination tones.

The manner of deriving D_4 is a little surprising, inasmuch as it might have been obtained in the equivalent form $(l - D_3)$, and then the theory would need to assume only the successive interaction of a combination tone just formed with one of the primaries, and never to call for the interaction of two combination tones. A further study of Hällström's procedure reveals an even more serious lack of systematic character.

His difficulties evidently were due to an uncertainty in dealing with algebraic quantities.

It will be seen that when the interval between the primaries is above an octave, so that $2l < h$, the value for D_2 will come out negative. Now it is plain that in applying formulas like these we may either adhere strictly to the rules of algebra, and retain all signs, or we may take only absolute values. As a negative combination tone hardly has a meaning, and Hällström had concretely in mind the number of coincidences or 'beats' produced in a second, he adopted the latter procedure. He seems not to have been clear, however, on the mathematical consequences of this position.

If in the example given we take simply the absolute value of D_2 we must then be cautious in the use of this value in further algebraic manipulations: it makes a difference in deriving D_3 as $(h - D_2)$ whether D_2 is positive or negative, and if we have neglected a minus sign in deriving D_2 as $(l - D_1)$ we shall get $D_3 = 2l$ and not $= (2h - 2l)$. The difficulty is even greater in the derivation of D_4. A study of Hällström's tables, which contain his calculations, shows that he was frequently confused, though perhaps with only dim awareness of being so. He dealt mostly with the smaller intervals, where his formulas hold strictly, but when he approached the larger intervals he often discarded the stated formulas and used others. He obtained D_4 not only as $(D_2 - D_1)$, but also at times as $(D_2 - D_3)$ and $(D_3 - D_1)$. There are other departures from his regular procedure, with even less reason for them. In short, Hällström did not work systematically, but at each level he manipulated the components already assumed to be present—primaries and combination tones alike—until he obtained a numerical difference that seemed proper in the light of his observations.

Hällström's system failed to gain acceptance, not so much on account of its mathematical uncertainties as for more fundamental reasons. Ohm (**9**) attacked it on two grounds. He denied the possibility of the successive interaction of tones, and he claimed that the presence of overtones in the stimuli

could account for all the combination tones as simple, first-order effects.

Ohm's views prevailed, and the problem that Hällström had brought forth was sunk in obscurity for half a century until it was raised again by the development of apparatus for the production of pure tones and the proof that multiple combination tones remain for such stimuli.

2. *Krueger's system.*—Krueger's (**5**) procedure for deriving the combination tones of higher order resembles Hällström's in many respects, but is more rigorous and seeks to be independent of theory. Like Hällström, Krueger obtained each order of combination tone as an interaction of components of lower orders; but he did not commit himself as to the actuality of the components or the form of their interaction. His procedure was made definite by the rule that the interacting tones at each level are always the two of lowest frequency among the components present.

Krueger's system is simple in actual use, but is difficult to present fully in symbolic terms. For small intervals the formulas are as follows:

$D_1 = (h - l).$	$(1300 - 1000 = 300).$
$D_2 = (l - D_1) = (2l - h).$	$(1000 - 300 = 700).$
$D_3 = (D_2 - D_1) = (3l - 2h).$	$(700 - 300 = 400).$
$D_4 = (D_3 - D_1) = (4l - 3h).$	$(400 - 300 = 100).$

These formulas are the ones frequently given in textbooks, but they are not general, except for D_1. The reason is that as the primary interval changes there are changes in the relative sizes of the resulting components, and hence in the pair of generators that must be chosen at any given level according to the rule. The expressions indicated hold for D_1 for all intervals, for D_2 up to the octave, for D_3 up to the fifth, and for D_4 up to the fourth. Beyond these limits other formulas come into play; and the necessary number of formulas increases with the order. For D_4, for example, there are six in all: $(D_3 - D_1)$ up to the fourth, as stated, $(D_1 - D_3)$ from fourth to fifth, $(D_2 - D_3)$ from fifth to major sixth and from tenth to twelfth, $(D_3 - D_2)$ from major sixth to tenth, $(l - D_3)$

from twelfth to double-octave, and $(D_3 - l)$ beyond the double-octave. Higher orders require an even larger number of expressions; perhaps from a systematic point of view it is fortunate that Krueger only seldom observed combination tones above the fourth order.

Krueger dealt only incidentally with summation tones; occasionally he observed the tone $(h + l)$, but no summation tones of higher order.

Both Hällström and Krueger were able in part to substantiate their methods of derivation of the difference tones by direct observation. Often one or more members were missing from the expected series, but these defects were tolerated in view of the difficulties of observation. A more serious fault in these systems is their failure to account for certain demonstrable tones. For example, Meyer (**7**) and Stumpf (**11**) got tones like $(2h - l)$ and $(3h - 2l)$, which are found in neither schema. Later work has revealed an even longer list of difference tones, and likewise a whole series of summation tones, to which these systems are inadequate.

3. *Stumpf's rule.*—In a brief outline of methods for calculating and naming the combination tones, Stumpf (**11**, pp. 38 *ff.*) mentioned the fact that all the combination tones may be derived by means of a single formula of the type $\overline{mh \pm nl}$, where h, l are the primaries, m, n are whole numbers, and the bar indicates that the absolute value is to be used. In this connection he gave the rule that the largest coefficient in the formula established the order of the combination tone. For example, the tone $(4h - 3l)$ would be of the fourth order, since 4 is the largest coefficient.

Stumpf does not mention the source of his formula or the rule which gives the order of the component. Both may be original with him. The formula resembles a pair of expressions developed by Bosanquet, of the form $mh \pm l$ and $h \pm nl$, but is more general: Bosanquet evidently did not recognize components in which both primaries enter in multiple relation. There is also a correspondence with Bosanquet's method of indicating the order: in Bosanquet's usage the value of m or n in the formulas indicates the order of the combination tone.

This method for naming the order of the combination tones has been but little used. Stumpf himself apparently did not employ it. It has recently been adopted by Newman, Stevens, and Davis (**8**), though without any mention of its source.

4. *The Helmholtz or 'transformation' system.*—This fourth system may be identified with Helmholtz's name, even though he himself did not employ it, for it arises naturally in the development of his theory of the origin of the phenomena through a transformation of energy of the primary stimuli into other frequencies (**2**; **3**, pp. 152 *ff.*, 411 *ff.*). As is well known, Helmholtz suggested that the combination tones arise through distortion in the ear; and the various orders of components are derived in the expansion of successive terms in the power series which describes the distortion.

This system has two distinct merits: it is based upon definite and rational assumptions, and it produces all the required components directly from the primaries. It is necessary, however, to modify the original theory in one respect. Helmholtz assumed that the seat of the distortion was the middle ear, especially the drum and the joints of the ossicles. Recent evidence has shown, however, that these structures are unimportant in this relation, since after breaking the ossicular chain and applying stimuli directly to the stapes, the same type of distortion pattern is found as under normal conditions (**12**). The most probable locus of distortion, therefor, is the inner ear.

As Schaefer (**10**) pointed out in an early statement of this conclusion, the principle of the Helmholtz theory of combination tones is in no wise impaired by a transfer of the distortion to the inner ear. It is true, this revision presents difficulties for the Helmholtz resonance theory, but that is another matter.

Specifically, the distortion theory postulates that in the vibratory movement of parts of the ear in response to strong stimuli the restoring forces are proportional not merely to the amplitude of movement, but also, in sensible degree, to the square of the amplitude. Helmholtz worked out the equations of motion to show that the simultaneous action of two

sinusoidal stimuli under these conditions will give rise to a long series of new components, overtones and combination tones, alongside the primary frequencies.

According to the fundamental assumption just stated, the response of sensory structures of the inner ear may be expressed by the following equation:

$$X = f(P) = a_0 + a_1P + a_2P^2 + a_3P^3 + \cdots,$$

where X is the resulting displacement, P is the imposed pressure, and a_1, a_2, a_3 . . . are constants. When two tones are presented, their sum

$$P = P_h \cos \omega_h t + P_l \cos \omega_l t,$$

where $\omega_h = 2\pi h$, $\omega_l = 2\pi l$; P_h and P_l are their respective amplitudes; and t represents the time axis.

If this value of P be substituted in the equation for X, we obtain the following results:

(1) The first term gives merely a constant.

(2) The second term, a_1P, gives $a_1P_h \cos \omega_h t + a_1P_l \cos \omega_l t$, which means that the stimulating frequencies, h and l, are heard. If all remaining terms were negligible we should have no distortion.

(3) The third term, a_2P^2, gives $\frac{1}{2}a_2(P_h^2 + P_l^2) + \frac{1}{2}a_2P_h^2 \cos 2\omega_h t + \frac{1}{2}a_2P_l^2 \cos 2\omega_l t + a_2P_hP_l \cos (\omega_h - \omega_l)t + a_2P_hP_l \cos (\omega_h + \omega_l)t$. Here the first expression is a constant, the second and third represent the overtones $2h$ and $2l$, and the last two represent the first order difference and summation tones $(h - l)$ and $(h + l)$.

(4) The fourth term, a_3P^3, gives, besides a constant, expressions for h, l, $3h$, $3l$, and the second order combination tones $(h - 2l)$, $(h + 2l)$, $(2h - l)$, and $(2h + l)$.

The mathematical procedure has been carried out by Cotton [1] as far as the seventh power of P, and gives a total of 42 combination tones. These are all the tones expressed by the formula $\overline{mh \pm nl}$ when m, n take all integral values

[1] As developed by Cotton, after Fletcher. See J. C. Cotton, Beats and combination tones at intervals between the unison and the octave, *J. acoust. Soc. Amer.*, 1935, 7, 44–50; H. Fletcher, *Speech and hearing*, New York: Van Nostrand, 1929, pp. 312 *f.*

within the condition that $m + n$ does not exceed the highest power of P. The total number of combination tones is obviously $(m + n)(m + n - 1)$.

In this system, as already suggested, the order of any combination tone is defined as one less than the power of P in the lowest term from which that tone comes. A simple rule is that the order $= m + n - 1$.

Studies on the nature of the distortion pattern in the ear, carried out on animals by means of the electrical responses of the cochlea, provide substantial support for the theory. Single pure tones give a long series of harmonics, at least as high as the 16th. Two pure tones give, in addition to the overtones of each one, a large number of combination tones. Members as high as the 20th order have been found under suitable conditions. These results indicate a power series which extends at least to P^{21}. Theoretically, so extensive a series should give combination tones to the enormous number of 420; but only a fraction of that number has yet been observed. The reason is that an upper limit is imposed on the frequency of the components by both the ear and the recording apparatus. The components that have been observed are all the earlier members indicated by the power series, and no components have yet been found that do not belong to this series (**13**).

It will be noted that in the definition given above the order of a combination tone is referred to the *lowest* term (in the equation for X) from which the tone arises. This specification is necessary because in the formulas as developed by Cotton each component is contributed to by various alternate powers of P. For example, the first-order tone $(h - l)$ is produced not only by P^2, but also by P^4, P^6, and so on. This is a serious complication, as it means that the coefficient of every component is an infinite, though doubtless a convergent, series.

It turns out, however, from a study of the electrical responses, that the actual relations of the combination tones to the stimuli are not nearly so complex as the mathematical treatment indicates. A given combination tone, within cer-

tain limits, varies in magnitude as a relatively simple function of the primary intensities. For example, the first order tone, $(h - l)$, varies as the product of the intensities of the two primaries, that is, as $P_h P_l$. The second order tone, $(2h - l)$, varies as $P_h^2 P_l$, and, in general, the combination tone varies in magnitude in a way determined by the manner in which the primary tones enter into its formation. Precisely stated, any combination tone, expressed generally by the formula $\overline{mh \pm nl}$, varies in intensity as $P_h^m P_l^n$. This relationship obtains for all combination tones that are present in sufficient magnitude for precise measurements, and for each it holds until at a sufficiently high level it becomes obscured by overloading.

This empirical observation means that the coefficients as obtained by the mathematical treatment are in error. The true coefficients should contain but a single expression, the first of the series as now constituted, which is the one yielded by the lowest power of P.

The form of the mathematical revision that must be made is not yet clear. It must be borne in mind that the solution of Helmholtz's original equation was only an approximation.

Despite the difficulties of mathematical detail, the basic implications of the distortion theory are in good agreement with the facts. The theory gives the required numbers of components, it places summation tones and difference tones on an equal plane, it obtains the overtones along with these components, and it indicates (even though incorrectly according to present derivations) the dependence of the magnitude of the components upon the intensity of the stimuli.

Other points of discrepancy between the theory and direct observation can be accounted for by subsidiary conditions. Certain conditions of selectivity evidently operate within the ear itself, in processes which accompany or follow the transformation process by which the new frequencies arise (**14**). Evidence has been obtained that such processes discriminate against the very low and very high frequencies. There is also the process of overloading, through which the response in the cochlea is limited in magnitude. This process refers to the

fact that at sufficiently high intensities the magnitudes of all components of the acoustic response no longer follow regular functions, but pass through a maximum and suffer a rapid decline.

Other conditions that show themselves in perceptual observations are doubtless of nervous origin. Masking probably explains the relative difficulty of hearing the summation tones, and also the difference tones whose frequency falls between the two primaries. The summation tones are necessarily higher than the primaries, and are likely to fall in the masking areas of them or their harmonics. The 'intervening' difference tone will often come in the masking area of both primaries; and hence will be difficult to detect. On the other hand, the difference tones that fall outside the masking areas will be heard readily. Those of low frequency, well below their primaries, are in the most favorable position; and this is the reason that the early observers heard only the 'grave harmonics,' and were unaware that higher combination tones are possible.

Conclusion

A comparison of the forms of designation according to the four systems is easily made by reference to Tables I and II, which are drawn up as representative of small and large intervals respectively. On the left in each table is indicated the composition of earlier members of the combination tone series, and on the right their orders according to the different systems. The listing follows the Helmholtz procedure, except that odd members are added at the end as necessary to complete the series for the first two systems. Only difference tones are given, since the first two systems are restricted to these.

Table I is for intervals below the fourth. Table II is representative of larger intervals, and actually is based upon the minor tenth, one of the two intervals beyond the octave for which Hällström gave his calculations. The last component of Table II, mistaken by Hällström for a combination tone, is of course an overtone. In this table also it happens,

TABLE I

DESIGNATION OF COMBINATION TONES FOR SMALL INTERVALS

Composition	Hällström	Krueger	Stumpf	Helmholtz
$(h-l)$	1	1	1	1
$(2h-l)$	—	—	2	2
$(2l-h)$	2	2	2	2
$(3h-l)$	—	—	3	3
$(2h-2l)$	3	—	2	3
$(3l-h)$	—	—	3	3
$(4h-l)$	—	—	4	4
$(3h-2l)$	—	—	3	4
$(3l-2h)$	4	3	3	4
$(4l-h)$	—	—	4	4
$(4l-3h)$	—	4	4	6

TABLE II

DESIGNATION OF COMBINATION TONES FOR LARGE INTERVALS

Composition	Hällström	Krueger	Stumpf	Helmholtz
$(h-l)$	1	1	1	1
$(2h-l)$	—	—	2	2
$(h-2l)$	2	2	2	2
$(3h-l)$	—	—	3	3
$(2h-2l)$	—	—	2	3
$(3l-h)$	4	3	3	3
$(4h-l)$	—	—	4	4
$(3h-2l)$	—	—	3	4
$(2h-3l)$	—	—	3	4
$(4l-h)$	—	—	4	4
$(5l-2h)$	5	4	5	6
$(3h-7l)$	—	5	7	9
$2l$	3	—	—	—

for the interval used, that Krueger's 4th and 5th order tones come out as identical.

For the last two systems the designations are independent of the primary intervals. These designations hold also for the summation tones of similar composition: the indicated orders will still be true if the formulas are read with plus instead of minus signs.

The method of designation of the combination tones as developed from the transformation theory is plainly to be preferred to the older systems. Hällström's system is based upon the unacceptable assumption of successive interactions, and also fails to account for numerous components. Krueger's

system suffers from the last-named defect, and it fails altogether to explain the phenomena. Stumpf's rule embraces all components, but it has no rational basis. The Helmholtz or transformation method, on the other hand, is systematic and inclusive, and places difference tones and summation tones on an equal footing. It is based upon assumptions which are not only reasonable in themselves, but have now been given a fair degree of experimental support.

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THE CONTRIBUTION OF PHYSIOLOGICAL PSYCHOLOGY [1]

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In a recent treatise on conditioning there appear the following statements: ". . . I venture to assert that no fact of the nervous system has as yet ever told anyone anything new about behavior, . . ." "It is toward the reduction of seemingly diverse processes to simple laws that a science of behavior naturally directs itself. At the present time I know of no simplification of behavior that can be claimed for a neurological fact" (p. 425).[2] I quote these remarks, first, because they constitute the serious and considered opinion of one of the most precise and profound thinkers in psychology to-day. In the second place, these statements are worthy of special attention because they represent the viewpoint held by a large number of psychologists—yet a viewpoint based upon a major misunderstanding as to the true nature of physiological psychology.

The contrast intended in the above statements is primarily between 'behavior' considered as overt response, and the internal activity of the organism. The point of view is that of the thoroughgoing externalist. I may add that I consider Professor B. F. Skinner—for the above statements are from his book, *The Behavior of Organisms*—one of the very few psychologists who has succeeded in maintaining a logically consistent viewpoint. But for all the numerous passages of careful inductive reasoning which characterize this book, and in spite of the major contribution to method which it repre-

[1] Read at the program celebrating the first semester of work in the new Life Science Building of the Department of Psychology in the University of California at Los Angeles.

[2] All quotations are from B. F. Skinner, *The behavior of organisms*, New York and London: Appleton-Century, 1938.

sents, I am convinced the author misconstrues the relationship between external and internal behavior.

To clarify the general viewpoint of the externalist: "Behavior is what an organism is doing . . .," ". . . behavior is that part of the functioning of an organism which is engaged in acting upon or having commerce with the outside world . . .," "By behavior, then, I mean simply the movement of an organism or of its parts . . ." (p. 6). Again (p. 423), "The very notion of a 'neurological correlate' implies what I am here contending—that there are two independent subject matters (behavior and the nervous system) which must have their own techniques and methods and yield their own respective data. No amount of information about the second" —(*i.e.*, the nervous system)—"will 'explain' the first or bring order into it without the direct treatment represented by a science of behavior" (p. 423). "I am asserting, then, not only that a science of behavior is independent of neurology but that it must be established as a separate discipline . . ." (p. 423).

I should first like to emphasize the fact that the procedure of labeling the conspicuous responses 'behavior,' and then classifying all other associated activities as physiological data which are improper subject matter for a psychologist, is purely arbitrary. The excuse offered for such a division is that "behavior is by far the more easily observed of the two" categories (p. 425). In recording the rate at which a rat presses a lever in a sound shielded box, Skinner is not observing behavior, but the end product of a series of movements. As Guthrie has pointed out, the rarest type of study in the field of psychology today is one in which the experimenter makes a direct observation of behavior. The procedure of most experimenters is to record the number of times a lever is pressed, a door opened, or the time it takes an animal to reach a food box. Obviously, the marking of a stylus on a kymograph at each lever pressing is only the roughest index of all the complicated activities concerned in the lever pressing. To appreciate how difficult it is to record external responses with precision, one need only consider the elaborate

equipment Carney Landis and his associates have assembled to photograph startle reactions in human subjects.

Do conspicuous responses or movements, taken as experimental data, have a unique distinguishing characteristic? Consider a theoretical situation like that arranged by Grindley in one of his experiments. An animal in a restraining cage is fed each time he emits a specific response. The particular response which is to produce food is arbitrarily specified by the experimenter. Suppose one takes an animal, such as a rabbit, that readily lies quiet within the restraining box. The animal has not been fed and may be assumed to be hungry. Each time, and only when, the rabbit blinks its left eye, food is automatically presented. Eventually the animal exhibits a blinking of the eyelid at a characteristic rate. This can be plotted just as the lever pressing of the rat in the Skinner box. In the same sense in which the rat's lever pressing is behavior, blinking of the left eyelid is behavior. The fact that the rat's lever pressing is conspicuous is merely due to a whim of the experimenter. The lever must be pressed a considerable distance if food is to appear. An experimenter could likewise insure a major lid movement by adjusting the recording system so that food was delivered only upon full lid closure. Suppose he increases the sensitivity of his recording system: he uses an electronic capacitance recorder which amplifies lid movements unobservable to the naked eye. Considering mere ease of observation, it is only reasonable to classify these covert responses as behavior. Suppose he continues, and next eliminates the effector, by extirpation. He places electrodes at some point on the efferent or motor pathways. Upon amplifying the action potentials he discovers a specific pattern of volleys which corresponds to the former lid closures. There is reason to believe such volley configurations could be plotted and classified in exactly the same manner as the lever pressings of the rat. In each instance the animal emits a variety of responses. Because of our particular limitations in sense organs we can see only conspicuous responses and not volley patterns. But in almost all experimental work the psychologist quantifies, generally by the aid of an instrument,

some portion of behavior which stands as an index of a total activity. With modern experimental techniques various 'internal' processes can act "upon and have commerce with" the outside world. It is true that the impulses in an isolated fiber might not be a valid index of lid closure. But a particular muscle need not play a consistent role in lever pressing even though it commonly exhibits contractions during such activity. I challenge the implication that there is a rational point of division between internal and external processes.

The real questions which must be faced, then, are: (1) Does the investigation of internal activity add anything to our understanding of behavior? (2) Is it true that the "science of behavior is independent of neurology . . . and must be established as a separate discipline . . ." (p. 423)? In challenging the assertion that "no fact of the nervous system has as yet ever told anyone anything new about behavior" (p. 425), I am starting from the assumption that no experimentalist can totally exclude the general method of correlation. If it is permissible to look for a relationship between two events within one sphere, *e.g.*, between a stimulus and a response, I assume that it is justifiable to seek a correlation between two entities on somewhat different planes of complexity, provided both are physical events in the same time-place continuum. For example, I assume that everyone agrees that the study of chromosomes has provided important data in the field of inheritance. Whether one feels that the facts obtained by by microscopic study and experimental manipulation of the chromosomes really 'explain' the data obtained by breeding, or promise important simplifications, is probably a matter of personal bias and the particular connotations aroused by the word, 'explain.' Explanation is little more than description which to a particular person suggests the futility of further research.

A geneticist could, I suppose, maintain a consistent externalistic viewpoint. He might perform certain operations, *i.e.*, make certain crosses in his strains, record his results, and not make any inferences as to common entities which accounted for the observed regularities. In noting that a red

four o'clock strain could be crossed with a white, and later isolated in an unchanged form, he would be soundly externalistic. But to make an inference that there are discrete determiners, which remain unchanged by this operation, is to depart from a strict externalism. A geneticist maintains a consistent externalistic point of view when he records the relative incidence of mutations within a particular strain. But when he makes the inference that in these strains there must have been a sudden and stable change in a determiner, he is no longer a strict externalist. The microscopic observation that a grossly disarranged chromosome accompanies the appearance of a mutation, provides a rough correlation of facts at two levels of complexity. Determiners are not thus directly observed. The correlation is at a crude stage. The microscopic approach did not lead to the initial discovery of mutations. Yet the microscopic data provide additional facts that the scientist cannot ignore. They must be taken into account in the interest of completeness if for no other reason. The internalistic approach may not provide the discovery of unique types of behavior—the externalistic approach has been exploited for thousands of years—but if it provides additional facts concerning behavior already in evidence, its exploitation is justified.

I should like to emphasize the fact that the distinction as to whether one reasons from the data of the breeding experiments or from the data of the microscope is unimportant: To postulate the discrete determiner as an entity is to employ the internalistic approach. A correlation has been made between the data at two levels of complexity. In a sense, the microscopic data may give somewhat more direct evidence of the discrete determiner as an entity. Direct observations may be somewhat more restrictive, in particular respects, than the inferences made from the breeding experiments when taken alone. Yet each interpretation involves inference. They differ only in the degree of inference. An electronic microscope may require more careful validation, as an instrument, than an optical system. There are more units which can give rise to artifacts. Similarly, an artificial lens system may require

more checks than would be necessary were the data obtained by direct visual observation. But even direct visual observation involves inference as to the normality of the experimenter's eyesight at the time of observation if the data are ever to be validated. The distinguishing feature of the internalistic approach consists in the attempt to provide a correlation between two facts at different levels of complexity. A particular formulation must not be classified as internalistic because it is the product of so-called physiological techniques or because it involves concepts that are the product of inference.

It is evident that a particular rate of after-discharge or a particular refractory period can be treated on a purely externalistic basis without any reference to a conceptual entity called the synapse. But a scientist cannot ignore the fact that this characteristic is a product of a particular locus in the neural substrate. No one correlation can adequately or completely 'explain' refractory period; but if one has any interest in completeness, and the essence of science is its inclusiveness, there is no justification for disregarding any relevant relationship. When the data show a relationship between facts on different levels of complexity, I do not understand how a scientist can avoid recognizing this correlation—even at the sacrifice of a consistent externalism.

I cannot accept the dogmatic claim that, ". . . a rigorous description at the level of behavior is necessary for the demonstration of a neurological correlate." To continue the quotation, "The discovery of neurological facts may proceed independently of a science of behavior if the facts are directly observed as structural and functional changes in tissue, but before such a fact may be shown to account for a fact of behavior, both must be quantitatively described and shown to correspond in all their properties" (p. 422). The correlation between the observation of a distorted chromosome and the appearing of a mutation is crude and only approximate. It would be ideal to show that a particular submicroscopic particle, in a particular location, is correlated with just one structural trait. It is perfectly legitimate to strive for a precise mathematical description of a phenomenon. But all

branches of science have begun with fragmentary observation. Pending the mathematical description, the scientist is not justified in disregarding any relevant fact no matter how isolated it may at first appear.

It is evident that in many instances a careful analysis of behavior at the external level may simplify a problem. For example, Norman Maier and Glaser have just published a study in which they have attempted to differentiate the 'convulsions' produced in conflict situations from the 'convulsions' produced by a unitary auditory stimulus. The former symptoms might be classified as the product of an experimental neurosis, the latter as a kind of epilepsy. Thus it follows that the latter classification suggests the presence of an internal factor in the nature of a structural defect which is not characteristic of all animals or all rats. The externalistic approach has doubtless simplified the problem to a certain extent in pointing to two types of factors which are related to the 'convulsions.' It is preposterous to insist that the two reaction patterns, assuming the behavior can be arranged in distinct classes, must be perfectly differentiated in all characteristics *before* exploiting the internalistic approach. The exploitation of such techniques as the recording of action potentials, stimulation of central and peripheral structures in 'normal,' *i.e.*, non-narcotized animals, the use of drugs, etc., could not help but clarify the external behavior by uncovering additional relationships.

Similarly, the correlation between cretinism and feeblemindedness is a very rough one. But the assumption that intelligence must be precisely described in all its numerous aspects before such a relationship can be sought is obviously fallacious. Cretinism does not 'explain' all retardation or feeblemindedness. It constitutes but a limited topic in the general field of intelligence. Yet no comprehensive discussion of feeblemindedness can ignore this relationship. And it is extremely doubtful that the most intensive study of feeblemindedness on an external level would disclose a distinct type of cretin-behavior as contrasted with other feebleminded performance. Likewise, the assertion that personality must first

be precisely described on an external basis implies that detailed analysis would disclose groups of traits which constitute meaningful entities. I am not convinced that such is the case. Rather, I would agree with Lashley that particular constellations of traits, provided by factor analysis, might only be made meaningful in the light of neurophysiological relationships. For example, I cannot believe that an intensive investigation of the manic-depressive reaction type on a purely external level would result in any sweeping simplification beyond that which we now have. It seems very probable that on the external level we have just about reached a blank wall even though there are minor details which are yet to be recorded. In those cases where the mood swing appears relatively independent of any external precipitating factor, it would appear that a fact on a lower level of integration may yet be discovered which will provide an important correlation. Such a finding would constitute a fundamental addition to knowledge of mood disorders in general, even though it did not 'explain' all cases now classified as manic-depressive psychoses. (Just how soon such a lower level fact will be discovered I would not presume to say. I am not sure it would be a feasible project to start a search for this hypothetical entity. There are other problems which offer higher probabilities of an early solution.)

As a more positive refutation of the claim that physiological psychology has nothing to contribute to behavior, I should like to discuss the problem concerning the rôle of the overt response in behavior. The overt response is a cardinal feature in the behavioristic formulation of learning, thinking, set, attention memory, emotion, etc. Consider the strong emphasis given to the discussion of the overt response, in current textbooks. Attention is described largely in terms of postural set. Thinking is described as a matter of muscle contractions. We can learn only those reactions we practice.

From the pedagogical standpoint, there is a certain justification in this emphasis. The average student is brought up to accept a mind-body dichotomy. Emphasis upon the overt response encourages the attitude of the experimental biologist

rather than the mentalist who conceives of behavior quite apart from structure. The early psychologists did not have available our present delicate recording systems with which to study neurophysiological activity. The emphasis upon overt response brought reassurance that one was dealing with material which could be objectively recorded. The data gave witness that one was a bona fide scientist.

Is the modern sophisticated psychologist justified in this elaborate emphasis upon the overt aspect of thinking, of emotion, attention, sensing? If the conspicuous responses are only an epiphenomenon of internal activities, and if the conspicuous activity of the organism is only an inaccurate indicator of a cerebral process, it is a serious error not to make every possible effort to study directly the primary mechanism.

It is my contention that it is most improbable that detailed externalistic studies of emotion will ever lead to an important simplification of the many evident diversities. Introspectively, anger differs from fear. The behaviorist, starting with this data as a hypothesis, proceeds to describe the conspicuous visceral responses which accompany a particular emotional state. But there is no consistent precise distinction which can be made on the basis of conspicuous visceral responses. Even the textbooks are beginning to admit this. But the authors of textbooks, being persistent behaviorists—or having tremendous inertia—claim that there must be distinctive postural adjustments which characterize each condition. Rage is typified by an approach response; fear by flight. I challenge anyone to show reliable objective evidence to validate this claim. Now if the conspicuous overt responses do not provide a clearcut distinction between anger and fear, I do not see a solution to this problem in an extensive tabulation of the various feelings that subjects can report, or an elaborate factoral analysis of these descriptions. However, there is reason to hope for a correlation between some distinctive neurophysiological process within the central nervous system and the verbal report, "I am afraid"; another distinctive cerebral process and the verbal report, "I am angry." The evaluation as to how much is already accomplished or

how much particular studies have contributed to such a correlation is irrelevant. It is conceivable that particular structures within the thalamus are not so important in providing this relationship as distinctive patterns of neural discharge, or, possibly, some electrotonic phenomenon. We are now beginning to master techniques for getting at some of the facts of such a correlation. Such a relationship would be of scientific importance even were it possible to distinguish the two emotional states on an external level. But the fact is that the externalistic approach has signally failed in making such a distinction. It is time this failure be clearly recognized. In three decades the external approach should have had a fair trial in attacking this problem of emotion. It has signally failed. The internalistic approach has not had an extensive test, nor can it ever be considered to have failed as long as there is the possibility of a new experimental technique. I do not see hope of a distinctly new technique for differentiating emotions on the overt level. I challenge the assertion that this approach warrants further extensive exploitation. In any event, I cannot believe that our only hope of a possible correlation between the data of the verbal report and the data of the neurophysiological substrate must await complete mastery at the external level.

As a second problem necessitating the internalistic approach I should like to discuss the topic of symbolic activity. To simplify the problem I shall consider symbolic behavior at the level of the delayed response. In the typical delayed reaction experiment an animal receives a stimulus either given by the experimenter or self administered. At a later period of time he is forced to give a specific reaction on the basis of that particular stimulus. It should be noted that this performance does not involve simple conditioning such that a unitary signal evokes but one consistent response. In simple conditioning there is commonly a one and one relationship. An external stimulus—or a peripheral stimulus, in the case of proprioceptive signals—leads to but one fixed response. But the delayed reaction problem involves a discrimination whenever more than one trial is given per day. And it can be

demonstrated that the stimulus is internal in the sense of being central rather than peripheral. In true delayed reaction, one stimulus, *e.g.*, a light, must evoke one specific response: a second signal must call out another specific response. To take a simple case, the animal must give an appropriate specific response according to which of the two signals occurs last. If a particular light came last the animal must make a left turn, if the other light appeared last it must make a right turn. My inference that the animal is reacting discriminatively is based upon the fact that if a rat is given but one trial per day he can apparently make a delayed response after a period of hours; if he is given ten trials his maximum delay is apparently about ten minutes. When more than one trial is given at a time the rat must identify that particular cue which came last. Hence the discrimination.

The first work in this field pointed to gross postural orientation as the basis of performance. According to this analysis, upon the appearance of the light the animal takes a position pointing to the correct exit. Orientation is maintained due to the inertia of the body. It may even fall asleep. When the restraining door is raised the animal makes a direct conditional response by running straight ahead. No true delay is involved. Some years ago I had the occasion to study the delayed response in connection with a project on cerebral localization. The behavior of the experimental animals, white rats, crucially refuted the externalist's formulation in terms of simple conditioning. During the delay period the rats would turn about as many as thirty or forty times. Careful tabulation was made of the number and pattern of turns. No correlation could be found between the conspicuous overt activity and the final response following the delay. Preceding a left choice on one trial, the rat might turn right fifteen times in horizontal rotation and ten times about a vertical axis. Description at the overt level utterly failed to provide a correlation between the behavior of the delay period and the subsequent choice. Having personally observed over 50,000 examples of such delay behavior, I am convinced that further study at the external level would fail to provide such

a relationship. Now it could be assumed that sustained minimal or covert movements served to span the delay period. Upon release, the animal need only give a simple direct conditional response to a proprioceptive stimulus of, *e.g.*, a neck muscle. The self initiated behavior actually exhibited by the rat was so vigorous, however, that such a hypothesis cannot be maintained. As an additional control some preliminary experiments were carried out with a general anesthetic, administered during the delay period. The data were too fragmentary to be more than suggestive, although there were signs the problem could be mastered under such conditions.

The past year and a half, Mrs. Ruth Hunter has continued the investigation of the possible effect of orientation, using an alternation box similar to that employed in the earlier study. She has carried out a very elaborate series of manipulations designed to destroy any maintained covert muscle pattern that would provide the immediate signal at the moment the animal was released from the delay box. Her rats mastered the distraction of being lifted while in the delay box and manually rotated about either the vertical or longitudinal axes. As one step in the project, the delay box with imprisoned rat was rotated while, simultaneously, the table holding the alternation apparatus was turned with respect to the room. Another procedure was to transfer delay box and rat in a random sequence from one alternation box to another on successive trials. As a further control, a motor was installed below the delay box. Rotation at a rate of approximately eighty revolutions per minute, or a speed which centrifuged the rat into a ball at one end of the cage, did not prevent mastery. As a final problem, an attempt was made to administer a general anesthetic during the delay period. Because of limitation of time it was not possible to run the animals long enough to show whether this condition could be mastered. One of the three rats was given only about half as many trials as he had required for his most difficult problem, and the other two rats received only about one-third of the trials required in their most difficult problems. Hence it cannot be decided at this time whether or not rats

are capable of giving a correct delayed response after relaxation of all skeletal muscles. Unfortunately, negative results could not be considered crucial since the excitement at anesthetization may distract the animal and prevent mastery. Regardless of the outcome of this final experiment (which is to be continued next fall) I am strongly of the opinion that the vigorous self initiated activity of the rat during delay, taken in conjunction with the successful mastery of the fast rotation, is crucial disproof of maintained covert contractions. Fast rotation rolls the animal in a ball, and elicits many active postural adjustments.

The above evidence, recorded at an external level, thus points, indisputably, to a correlation between final choice behavior and some entity of the neurophysiological substrate. I am not content to note merely that the rat can turn correctly after a period of delay. If there was justification for the externalist's attempt to correlate the final choice with overt posture of the delay period, there is similar reason to seek a correlation between events at different levels of complexity. Had the behaviorist's original interpretation been verified in this problem, there would have been a simplification in referring the behavior to the class of simple conditioning. But the attack on the present problem has utterly failed at the external level, as shown by the crucial behavior of the delay period. (It may be noted, in passing, that the attempt to press a correlation between the choice behavior and the hypothetical covert movements is not, strictly speaking, an externalistic operation—according to Skinner's definition of behavior. But as it was pointed out above, there is no logical basis for a sharp distinction between conspicuous overt movement and minimal covert movement. With this continuity in mind, we can classify the proprioceptive stimuli as peripheral events and no question of consistency arises.)

Because correlation between the overt choice behavior and the alleged covert posture is so crucially opposed by the evidence, one is forced to seek a relationship between the choice behavior and an entity of the central nervous system. The entity is derived by inference. Just as the geneticist

was led to postulate underlying determiners, so the psychologist is forced to make the present inference. Specifically, the inference is that the basis of the delayed alternation performance is a discriminatory reaction to the most recent of two or more reactivated neural traces. The assertion that the signal consists in a reactivated neural trace is based upon the evidence which decisively showed that no gross or minimal muscle contractions were maintained through the delay period. (It appears very improbable, although not inconceivable, that a peripheral sense organ could have maintained a continuous discharge throughout the delay to provide the final cue.)

The externalist may feel that such a correlation with the nervous system is not very restrictive. But the justification for the internalistic approach, *i.e.*, the attempt to correlate the final choice behavior with an entity of the central nervous system, does not depend upon the magnitude of present achievement. If the externalistic description of the conspicuous delay behavior has reached a blind alley, and I see no alternative to such a conclusion, the only hope of further simplification lies in an attempt at an internalistic correlation. Either one must summarize the data by saying the animal made a correct conditional response following a period of delay; nothing more can or needs to be stated about the problem—since everyone is familiar with simple conditioning; no further simplification can be hoped for, therefore we may as well give up further research; or, one looks for additional facts at an internal level which will permit further correlation. Many problems in the field of symbolic behavior will require unique or radically improved techniques if they are ever to be solved. But there are numerous experiments which even now lie within the realm of feasibility.

I had discovered in my experiments on cerebral localization that relatively small lesions in the frontal lobes of the rat's cortex produced striking loss in capacity for delayed reaction. And it is known that organisms with relatively little frontal cortex show a limited capacity for multiple delayed response. C. F. Jacobsen and his associates have since verified this correlation in primates; they have explicitly demonstrated

that lesions in other parts of the brain do not produce a comparable loss in capacity for delayed response. Such a correlation is relatively crude and yet the internalistic approach suggests the outline of further investigations, whereas the externalistic approach would ignore certain relevant facts and suggest that further study is impossible.

Quite aside from the immediate significance of this work, there are some important general implications. The process involved in the delayed response can be classified as symbolic. That is to say, the rat does not make a simple direct conditional response to an external stimulus. He cannot respond directly to the signal light because it is not present in the environment at the time of his choice. There is neither gross nor covert posture which can serve as the signal or cue. Hence the rat must be responding to a reactivated neural trace, aroused at the time of choice, and serving as a symbol or substitute for the antecedent external light. According to this analysis, the behavior of the delay period is an epiphenomenon which does not accurately or reliably indicate the nature of the trace being carried within the central nervous system. If, in the rat, delay behavior is an unreliable index of the underlying trace, there is every reason to believe that the movements exhibited by man, in connection with similar symbolic performances, are also an epiphenomenon. And it is evident (to anyone but the consistent externalist) that overt behavior is not infrequently a most misleading index of the underlying symbolic activity. In other words, to make the assumption that correlated with all symbolic behavior there must be significant muscle activity, may be a worthy exercise for the introductory student. Since the experimental evidence does not disclose these maintained movements in particular cases, and in the present instance crucially disproves their existence, it seems the duty of the modern psychologist to abandon such primitive constructs.

In Watson's time there was an understandable motivation for an overemphasis upon the conspicuous responses of the organism in thinking and attention. There were not then available suitable techniques to study neural activity while

the subject was in a normal, may I say, conscious, state. To maintain face as an objective scientist, the behaviorist felt it imperative to show a correlation between the verbal report of fear and distinctive responses of the viscera. To justify the claim that symbolic processes could be studied objectively, one had to find a correlation between the solution of a problem and the intervening postural activity. Attention, as verbally reported, had to be correlated with overt or minimal movements. Learning and problem solving consisted in the conspicuous movements of the animals in the problem box. Were this assumption invalid, the behaviorist of Watson's era would have had to admit that emotion, thinking, attention, and learning could not be studied objectively and hence that he was not much of a scientist.

It is time this grossly exaggerated emphasis upon the conspicuous muscular adjustments be abandoned. The modern psychologist no longer is so insecure that he has need of such primitive crutches. He knows there is a correlation between the verbal report of hearing a tone and an internal neural substrate *which he can investigate.* Whether he feels that the correlations now worked out satisfactorily 'explain' hearing is irrelevant. I cannot believe that another generation of externalistic description of sensation must preceed the exploitation of the internal approach. The data of neurophysiology add to the sum total of knowledge; they cannot be ignored. Our understanding of the general data of hearing has profited enormously by a simultaneous exploitation of both the internal and external approach as typified by the work of Wever and Bray, and Culler and his associates. I seriously doubt if anyone to-day can adequately appreciate the full possibilities and ultimate contribution of the spinal conditioning procedure, as outlined in the brilliant experiments of Shurrager and Culler.

The field of learning in general and conditioning in particular would seem to necessitate a combined attack of an external and internal approach. It is evident that the relatively meager progress in the field of learning up to recent years is due to the fact that experimentalists have tended to

ignore internal factors such as set. It has been recognized since the time of Aristotle that a stimulus which accompanies a response tends later to evoke that response. Is this as far as we can go in understanding this particular law of association? What of the apparent exceptions to this rule? One great weakness of the external approach is that it cannot account for the exceptions arising out of 'self induced' states which have only a very remote connection with the operations of the experimenter. The externalistic approach is adequate to work out the correlation between a particular button on a radio and the program of a particular station. It requires an internalist to understand the apparent exceptions. A vacuum tube may burn out very prematurely due to some hidden flaw within its filament. The internalistic approach is obviously the most direct way of understanding why a particular button operation no longer produces a particular radio program. In the field of conditioning there have appeared numerous examples in which a stimulus has accompanied a knee jerk but the knee jerk has failed to be conditioned. Numerous experimenters—Razran, Hilgard and his associates, and others—have shown the importance of set in determining the properties of a conditional response. Now as a consistent externalist one is justified in reporting the instructions which affect the process of conditioning. But to account for apparent exceptions to the simple laws of association by invoking postural set is to adopt the internalistic approach. To make such an inference is, I believe, not only desirable but unavoidable. Had the early behaviorist been able to demonstrate a reasonably high or reliable correlation between particular instructions and a particular postural substrate he could have remained, in essence at least, an externalist. But notwithstanding early optimism as to a reliable correlation between the report of an image and the pattern of action potentials at the appropriate muscles, recent work refutes this first interpretation. I would challenge the claim that there are conspicuous postural adjustments that correlate *reliably* with the verbal report of a particular set, image, or state of attention. If the conspicuous behavior which does accompany the verbal

report of a set, an attitude, a state of attention can thus be shown to be an epiphenomenon of the reported state, one must either ignore the obvious inference that set does account for the observed differences in conditioning, or look for the entity within the central nervous system.

The major weakness of a consistent external operationalism is that a description of the experimenter's manipulations does not, in particular situations, reliably identify particular internal states that play a major rôle in the subject's behavior. Since it is not practical, assuming for the moment it were theoretically possible, to record the complete detailed biography and heredity which determine the 'set' or attitude assumed by an organism, the solution of such problems must obviously await a direct measurement or index of the internal state. In dealing with inanimate material the chemist or physicist can designate a particular entity by merely describing his experimental operations. Another investigator can identify the same entity and validate earlier findings by repeating the original manipulations. The outstanding aspect of psychological material, and a feature overlooked by the consistent externalist, is that in particular situations an organism assumes a 'set' or attitude, of major importance in determining his behavior, which is to a very large degree independent of the experimental manipulations in that particular problem. This set cannot be identified by the most meticulous description of experimental routine; it cannot be identified by the most detailed description of the postural 'set'; it is not possible to identify the 'set' from the subject's biography or heredity; hence, the only possible solution of such problems lies in a direct measurement of some reliable index of the neurophysiological state within the central nervous system which constitutes this set. I would not ignore the fact that there are numerous situations where a description of the experimental operations does identify the relevant internal states with reasonable reliability. But in particular problems, the understanding and prediction of behavior must remain indeterminate until methods are devised for obtaining a direct index of the organism's internal state.

The present limitation of knowledge regarding the properties of the nervous system is in no way a justification for abandoning the internalistic approach. I grant that much of this data is as yet fragmentary. But it must be realized that, in contrast with neurology or the anatomy of the nervous system, the direct study of neural activity in unanesthetized animals and humans began only within the present decade. Surgical techniques have been used for a longer period of time, but for the most part extirpation has not been applied to functional units of the nervous system. It is evident that the internalistic attack of the future will employ a battery of procedures in which techniques of electrical stimulation and recording, surgery, narcotization, and conditioning all contribute material for one project.

It is extraordinary that anyone should urge the rejection of a particular correlation because it falls short of a perfect one-to-one relationship in every detail. Consider the somewhat fragmentary observation, reported by Girard: as an animal goes under a general anesthetic, sensory impulses initiated at the skin continue to arrive at the cortical projection cells up to the time the animal is about to die. In contrast, the basic, one might say, self maintained, or emitted volleys of these cells slow down step by step with increasing narcotization. This is obviously an important relationship. Interpreted in conjunction with Penfield's results on cortical stimulation of conscious patients, it appears that loss of consciousness is probably not due to the failure of discrete sensory volleys to reach the brain, but to a change of the 'autonomous' rhythms within certain central structures. This in itself may not impress the externalist as being a particularly restrictive inference, but there is no reason why many more additional facts of this general type may not eventually be accumulated. Ultimately, it should be possible to make a correlation between the retrospective report of sensations while losing consciousness and numerous physiological data. And it is not inconceivable that electrotonic phenomena, reported by Girard and others, may ultimately provide a fundamental correlation with data of a more complex level such as

those of conditioning. (The concept of electrotonus is that of one neural system acting through a distance—not serially, along neurones—so as to influence the activity in a parallel system of fibers.)

A brilliant discovery involving both internal and externalistic correlations is the finding that ultraviolet irradiation of the retina, in certain species, produces changes in the anterior pituitary and gonads, followed by migratory behavior. A strict externalism is limited to a formulation of the relationship between the radiation and the migratory behavior. Knowledge of the general phenomena of migration contains serious gaps when the internal humoral linkages are disregarded. And the most elaborate quantification of the conspicuous migratory behavior would fail to account for particular instances. The case where one bird fails to migrate with others of its species might, on occasion, be adequately accounted for in the absence of elaborate quantification by merely describing the functioning of its humoral systems.

In the near future many new relationships will inevitably be worked out between such behavior as mating, nesting, retrieving of young, hibernation, and the action of particular hormones upon specific neural mechanisms. It is my contention that there can never be a reasonably comprehensive description of drive and appetite until the internal mechanisms concerned with such behavior are worked out. If there is any justification for the attempt to describe the relationship between external stimuli and conspicuous responses there is every reason to study the relationship of behavior to internal events. What are the specific stimuli which give rise to various appetites and thirsts? What are the essential mechanisms underlying various 'tissue cravings'? Inasmuch as it is clearly evident that the externalist's effort to show a correlation between a verbally reported emotion and distinctive movements has reached a blind alley, and since it is apparent that similar attempts to correlate symbolic activity, attention, set, with a postural substrate have also decisively failed, there is not only every justification but an imperative need for a more extensive development of the internalistic approach.

These externalistic studies have served an invaluable purpose. It would have been unjustifiable to assume the lack of correlation without these elaborate experimental studies. Now that the negative findings in these particular fields have been accumulated so extensively it would seem the most expedient measure to exploit as fully as possible the internalistic approach.

There is reason to believe that in many fields a vigorous attack on the external level will provide important relationships within the one sphere. Propaganda analysis, public opinion polls, complex social behavior—these can hardly be considered the most fruitful projects for the internalist. The amount of time and effort one expends upon a particular approach will, of course, be a product of his background, his skills, his emotional bias in favor of one type of work rather than another. Scientifically it is as futile to argue that one approach is more valuable than the other, as it would be to insist that, the world being what it is to-day, one should study medicine, or preach brotherly love, or go in for politics. This is obviously not the place to evaluate all the arguments in favor of applied as opposed to 'pure' science.

To summarize the thesis of this paper: There are a variety of fields, *e.g.*, learning and conditioning where there remains so much to be studied that, on purely practical grounds, due to limitation of time and energy, a particular investigator is perfectly justified in restricting his efforts to an externalistic approach. (I am not so confident, however, in view of the tremendous complexity of these fields, that an effort to provide a detailed quantitative analysis will be so fruitful at the present time as the attempt to identify various 'qualitative' factors. This is, of course, a matter of personal bias and is obviously open to debate.) On the other hand I would challenge the implication that there are logical reasons why conspciuous responses *must* be studied independently of internal activities and completely quantified before attempting to study the correlation of particular behavior with the accompanying internal activities. I, personally, am impressed with the large amount of behavior which is influenced to a major degree by internal states only remotely connected with the

operations of the experimenter. Understanding and prediction of such behavior can never be achieved through the externalistic approach. In particular problems, therefore, the internalistic approach provides the only reasonable hope of a simplified formulation.

From the purely empirical viewpoint, in the light of the history of scientific development, there are a number of arguments which might be offered for the combined and parallel attack on a particular problem by a variety of techniques rather than an intensive development of a very restrictive approach. In a sense, the restrictive viewpoint of the externalist is somewhat more characteristic of the applied scientist than the so-called 'pure' scientist. The technologist, commonly, is interested in regularities, whereas the 'pure' scientist often tends to be attracted by the apparent exceptions, *i.e.*, the mutations, or the individual cases where conditioning does not appear to fit the general law of association. The differences in viewpoint between the externalist and the internalist depend in part, therefore, upon a personal and special interest in regularity as opposed to apparent variation.

Upon one last point I am constrained to adopt a very dogmatic stand: there is so little that is known about certain fields of behavior, *e.g.*, learning, so much that is yet to be determined, that psychology is not yet in a position to assert that in such cases one particular project or line of investigation must on logical grounds inevitably take precedence over all others.[3]

[MS. received July 3, 1940]

[3] I should like to express my very great indebtedness to Professor Skinner for his numerous helpful comments on this paper, which have led me to elaborate and, I trust, somewhat clarify several points in my original manuscript.

THE LOGICAL CHARACTER OF THE MOTIVATION CONCEPT. II

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I. Implicit Definitions of Motivation

In the previous article [1] we discussed only the operational definition of the motivation concept. We saw that the logical form of such definition is given by the conditional reduction sentence, and we equated the factual meaning of the concept with the totality of its reduction sentences.

Operational definition, as we previously showed, is only one small step in the process of theory construction. No matter what degree of precision we attain in the operational definition of our concepts, nothing resembling theory begins to appear until certain functional relationships between the concepts are asserted. Theory appears when these laws or assumptions (usually both) are stated in such a way that testable consequences may be deduced and subjected to experimental verification. We have called this process of throwing the sentences relating certain concepts into the form of a postulate set, *implicit definition.*

It will now be our task to inquire into certain of the implicit definitions of motivation. Implicit definitions are to be found in the theories of motivation which embellish the literature of psychology.[2]

There is a kind of wan humor in calling most psychological theories in their present form 'implicit definitions' or 'postulate systems.' Most psychological theories have been formu-

[1] The logical character of the motivational concept. I. Psychol. Rev., 1941, 48, pp. 15–39.

[2] Explicit definitions may also be advanced for certain of the terms in the postulate set. Much of the 'progress' in the field of motivation has unfortunately consisted *solely* of elaborate substitution and counter-substitution of new symbols. Psychologists have not been nearly so fertile in proposing new implicit definitions, or systematizing old ones.

lated in a hasty, crude, ambiguous manner. Postulates are rarely explicitly stated. The closest approach to deductions are usually vague, intuitive guesses. It is frequently difficult to distinguish postulates from theorems, explicit definitions from implicit definitions or laws, empirical constructs from formal terms. It is sometimes a moot question as to whether experiments supposedly confirming or refuting a given thesis have any demonstrable relation to that thesis. Whatever can be said to correspond to postulates are usually put forward in loose, verbal, haphazard form. While the empirical research carried on by psychology has been elaborately experimental ever since the time of Wundt, the form of its theories remains just as elaborately literary as in the days of Aristotle.

Thus we approach our logical analysis of the implicit definition of motivation with some trepidation. Nothing can be gained from a comprehensive historical précis of motivation theory. Instead we will concentrate our attention on two theories representing varying degrees of logical sophistication. We have chosen the motivation theories either explicit or implicit in the work of Clark L. Hull and Kurt Lewin.

Several considerations have dictated this choice. For one thing, both psychologists are among the most advanced theorists in the field. For another the theoretical emphasis of each corresponds to the chief methodological notions under analysis. Both Hull and Lewin have been pioneers in employing postulational technique in the elaboration of their theories. Hull's procedure, however, is of the telescopic variety, while Lewin's comes close to the interpretive.

Since an empirical postulate set has little meaning in isolation from the operational and explicit definitions of the terms which it relates, we will not attempt to deal solely with whatever corresponds to the postulates (implicit definitions) in these theories. And since we are interested in motivation theory mainly as a locus of concreteness for our discussion of logical issues, we will not restrict ourselves to a discussion of the possible motivation theory lurking in the work of our two exhibits.

Thus, our primary purpose in the sequel is to survey briefly the procedures of Hull and Lewin from the point of view of technique of theory building.

II. Hull and Telescopic Procedure

The work of Clark L. Hull represents one of the closest approximations to rigorous scientific theory that has as yet appeared in psychology. For this reason it will be interesting to survey his procedure, both with respect to the concrete problem under investigation and with respect to the general outlines of his theoretical constructions.

The missionary fervor which Hull has applied to the popularization of postulational technique among psychologists has been one of the most healthful influences in psychology of the past decade. The rosters of psychology would still abound with those iron-hearted empiricists who object to the very mention of theory and revere pure, ineffable fact, if it were not for the pioneering efforts of Hull and his followers.

Unlike most theorists Hull is not a systematizer of the heroic school. Many psychological theorists, in their anxiety to achieve a complete and rounded psychology, have had to sacrifice theory for program.[3] Their work has consisted of the development of concepts presumably adequate to the whole field of psychology, and the prophetic adumbration of certain vague relations holding between them. Hull proceeds much more realistically. Realizing that the smallest theory having some inferential relation to its data is preferable to the vastest speculation related to its data only in the intuition of the theorist, Hull has theorized in a cautious, step-wise manner. And instead of spinning plans for the future, Hull has actually elaborated several 'miniature' psychological systems which are miracles of rigor in comparison with what usually goes under the name of theory in psychology.

[3] Freud, McDougall, and Tolman may be taken as widely divergent examples of this tradition. It is not their exclusive property. Literary psychology of all ages can boast, at best, only this programmatic utility. And the distinction between the most flagrantly literary psychologies and the systematically most advanced is, in this sense, merely one of degree. Even the system of such a methodological sophisticate as Lewin is largely a blue-print for the future.

We are doomed to disappointment, however, if we expect to find even a reasonably complete implicit definition of motivation in Hull. This should not surprise us, for we have already noted that a complete implicit definition of motivation presupposes a complete postulate set [4] for general psychology. But Hull has never specifically concerned himself with the problem of motivation either in general discussion or in any of the 'miniature' systems [5] he has put forward. The motivation constructs and principles which he has developed to date, enter into his work only incidentally as necessary mechanisms for the explanation of behavior. Hull has concentrated his interests largely in the field of animal learning, and he has introduced his motivation constructs primarily in connection with his work in this field. Let us see what they look like.

The drive-stimulus.—The two Hullian constructs most directly related to motivation are: (1) the 'drive-stimulus' (S_d) and (2) the 'fractional anticipatory goal response' (r_G).

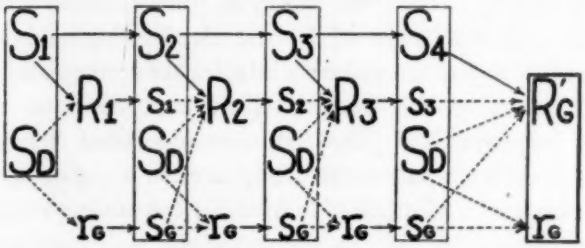
S_d was first introduced in Hull's article 'Knowledge and purpose as a habit mechanism' (3).[6] In this article, S_d is characterized as a "persisting core of sameness in the stimulus complexes throughout the successive phases of the reaction sequence." Examples are: "a continuous strong, red light, or a continuous gripping of a dynamometer, or the continuous knitting of the brow, or (more typically) the continuously recurring crampings of the digestive tract as in hunger" (3, p. 519).

[4] By a 'complete' postulate set, we mean one capable of generating theorems adequate to the description of all phenomena in the field to which it pertains. Completeness is one of the three criteria which an ideal postulate set is expected to satisfy (the others are consistency and independence). Katsoff (10) has scolded Hull for not attempting to demonstrate the completeness of the postulate sets which he uses. But, unfortunately, there is no *a priori* technique for proving the completeness of empirical postulate sets; one can only keep on patiently deriving and verifying theorems.

[5] Hull's three published 'miniature systems' have been devoted to: (1) rote learning (6); (2) adaptive behavior (7); (3) stimulus equivalence (8). A fourth, but not so very miniature, system on rote learning (9) has appeared since this article was written.

[6] 'S_d' was called 'S_p' in this article. Hull had equated 'S_p' and 'purpose' at this point. In a later article (4) Hull renamed 'S_p,' 'S_d' and equated purpose not with 'S_d' but with 'r_G.'

The functional significance of S_d can best be represented by means of the figure adapted from Hull (4). S_1, S_2, etc. are a



series of stimuli corresponding to some environmental sequence. They evoke reactions R_1, R_2 etc. S_4 corresponds to the goal stimulus and R'_G is the goal reaction. Each of the reactions in the series sets up proprioceptive stimuli s_1, s_2 etc. Since these proprioceptive stimuli occur simultaneously with the environmental stimuli evoking the next reaction, an associative bond (indicated by dotted arrows) tends to be formed between each s and the succeeding R. This follows from the operation of a principle of redintegration. S_d, however, is present all through the sequence. And thus, again by the principle of redintegration, S_d gets associated with *every* response in the series.

The immediate context in which Hull introduced S_d was in connection with an attempted solution of the problem of how the "dropping out of undesirable behavior segments may come about, since it appears to be a violation of the 'law of use'" (3, p. 524). Suppose that, for some reason, the environmental stimulus sequence is blocked after presentation of S_1. If the excitatory tendency of the proprioceptive s's to evoke the R's is sufficiently strong, the entire response series will run off even in the absence of the remainder of the environmental sequence. For Hull, this ability of the proprioceptive s's to serve as surrogate for the environmental stimuli is the elementary counterpart of knowledge. But the chain of R's preceding the goal reaction may possess little 'instrumental' [7] significance. That is to say it may be com-

[7] An 'instrumental' act is one, the full overt performance of which is a necessary condition for the organism's reaching a given goal.

posed of 'pure stimulus acts,' the sole function of which is to serve as stimuli for other responses. In such a case, biological economy demands that the chain of pure stimulus acts intervening between delivery of S_1 and the final goal reaction be eliminated. S_d proves to be an admirable conceptual mechanism for attaining this result. For, according to the 'goal gradient hypothesis' [8] S–R connections tend to get more strongly associated the closer they are (in time or space) to a 'reinforcing state of affairs.' And S_d, becoming associated as it does to every reaction in the sequence, will tend to develop strongest excitatory tendency with respect to the evocation of R'_G. Now if the excitatory tendency of $S_d \rightarrow R'_G$ is greater than the combined excitatory tendencies of the S's *plus* S_d to evoke any reaction earlier in the sequence, then the pure stimulus chain $R_2 \rightarrow s_2 \rightarrow R_3 \rightarrow s_3$ will drop out of the series, and R'_G will occur immediately on presentation of S_1. This, in substance, is Hull's solution to the problem of how biologically 'unnecessary' acts are eliminated.

The primary functional significance of the S_d construct is indicated by Hull in the following passage:

> The unchanging stimulus component (S_d) gives the various action segments of the behavior flux its characteristic unity by tending largely to limit the behavior to acts which in the past have been associated with food (or any other goal) and which are therefore more likely than pure chance to result in securing it again. And when at length the food is found and eaten and the digestive crampings have ceased, the food-seeking behavior naturally comes to an end because its stimulus motivation has ended (4, p. 487).

The chief difference between Hull's S_d and the host of other drive concepts [9] current in psychology is that S_d can be any persistent stimulus source (internal or external), whereas 'drive' is most often treated as an internal source of stimulation depending upon the physiological condition of the organism.

[8] A form of this is presupposed in 'Knowledge and Purpose as Habit Mechanisms' (3) but the hypothesis was not specifically stated and elaborated until a later article, 'The Goal Gradient Hypothesis and Maze Learning' (5).

[9] Young (17) has distinguished six meanings of 'drive.' Sufficient patience will disclose about a dozen more.

The anticipatory goal response.—The concept of the fractional anticipatory goal response (r_G) arises from the commonplace observation that after a behavior sequence has been rewarded, it typically takes on a 'purposeful' character. Such purposeful behavior is frequently characterized by anticipatory movements, *e.g.*, the tendency of young children to salivate as a food goal is neared. Hull derives the functional properties of r_G in the following way: Assume a behavior sequence identical with the one diagrammed in the figure, except that in this case the series R_1, R_2, etc. has a genuinely instrumental significance. If the response series is genuinely instrumental (*e.g.*, the running movements of a rat in a maze) the displacement of the earlier members of the series by the goal response (as in the case of the pure stimulus chain) would be biologically maladaptive. But the presence of S_d with its tendency to evoke the goal response at the beginning of the series, would tend to produce this maladaptive result. Observation indicates, of course, that this is normally not the case. The difficulty is dispelled by conceptually analyzing the goal response into two components: R'_G and r_G. The anticipatory goal response (r_G) may be understood as that component of the total goal response which is not incompatible with the instrumental acts earlier in the sequence. It now follows that in instrumental sequences S_d will tend to bring to the beginning of the series not R_G in toto, but the r_G component of the goal response which is not disruptive to the performance of the response sequence. Having made this step of introducing r_G, we now find ourselves in possession of a very powerful behavioral construct. For r_G provides a proprioceptive stimulus s_G which, like S_d, acts as a second persisting core of stimulation throughout a behavior sequence; and like S_d, it will tend to get associated to every reaction in the sequence.

The peculiar functional potentialities of r_G and its correlated s_G may best be brought out by contrast with S_d. Despite their important similarities they differ in the following respects: (1) They differ in origin. S_d typically has its origin in physiological processes. The anticipatory goal response,

however, is "dependent on the existence of the drive stimulus and the conditioning of it to the goal reaction." "The drive stimulus is not likely to be greatly disturbed by either the presence or the absence of the goal stimulus, but anything which terminates the drive stimulus will automatically bring the goal stimulus to an end." (2) Many distinct goals can arise from a given drive stimulus. A hungry rat, for instance, may eat many different kinds of food. "The eating of each kind of food may become a different goal, with the goal reaction in each case . . . distinct" (4, pp. 496–497).

On the basis of the properties of r_G and s_G, Hull goes on to identify ideo-motor action with the anticipatory goal reaction. Hull points out that most psychologists of the past have looked upon ideo-motor action as a maladaptive curiosity. Once equated with r_G, ideo-motor reactions shape up as 'guiding and directing pure stimulus acts' which perform the 'enormously important function ordinarily attributed to ideas' (4, p. 502). "The anticipatory goal reaction seems also to constitute the physical basis of the somewhat ill-defined but important concept of purpose, desire or wish, rather than the drive stimulus as has been supposed, notably by Kempf" (4, p. 505).

The logical form of S_d and R_G.—Let us now attempt to throw Hull's procedure in the elaboration of these concepts into line with the canons of scientific theory that we have been using. S_d, r_G, and s_G, taken by themselves, are scientific constructs that Hull has introduced for the purpose of explaining (deducing) certain behavior phenomena. In the first place, we may notice that they are given explicit definition: 'S_d' being replaceable by some such expression as 'persisting stimulus throughout a behavior sequence', 'r_G' by 'that component of the total goal reaction which is not incompatible with the performance of an instrumental behavior sequence', and 'S_G' by 'the persisting proprioceptive stimuli evoked by r_G.'

The second thing we should notice is that besides introducing these constructs by explicit definition, Hull also attributes certain functional properties to them. For instance:

S_d tends to get associated with every response in the series; r_G is brought forward to the beginning of the sequence by S_d; and s_G also tends to become associated with every response in the series. How do these properties happen to emerge? Do they spring full-grown out of the explicit definitions of these constructs? Examination of the above account will disclose that the properties imputed to S_d, r_G, and s_G are logical consequences of (1) certain principles (postulates) that Hull assumes and (2) the explicit definitions of these and certain other constructs (S, s, R, R_G, etc.). The postulates that Hull assumes in his informal derivations are: (1) a principle of 'positive association' (or 'conditioning' or 'reintegration' or 'reinforcement'), (2) some form of the 'goal gradient' hypothesis and (3) a postulate about 'excitatory tendency' to the effect that given several competing S–R connections, that stimulus with the strongest excitatory value is the one most likely to be effective. Given these postulates and definitions, it is quite easy to deduce the major properties of Hull's motivation constructs.

Implicit definition and Hull's motivation constructs.—We must not forget that we are still looking for whatever corresponds to Hull's *implicit definition* (postulate set) of motivation. We have found three constructs which appear definitely motivational in character. We have also found certain statements about the functional potentialities of these constructs, and we have seen that they are derivable, in an informal manner, from three principles and a group of explicit definitions. But now we face an interesting outcome. On the basis of our account of theory, we must identify the implicit definition of motivation with a postulate set consisting of principles which, on inspection, do not seem to have any direct relevance to motivation. This is only another indication of the fact that if we want a theory of motivation which is somehow independent of principles relevant to fields other than motivation, we must join hands with Troland (15, 16) and Young (17) in equating 'motivation' with general explanatory psychology.

The r_G and S_d constructs were introduced quite informally

in the articles which we summarized; in his miniature system on 'adaptive behavior' Hull presents a formal derivation of the following theorems:

> X: A fractional anticipatory goal reaction as a stimulus will tend to bring about the reinforcing state of affairs with which the total goal reaction of which it is a constituent part, is associated.
>
> XI: Organisms capable of acquiring . . . anticipatory reactions intimately associated with the reinforcing state of affairs, will manifest a weakened tendency to the consummatory reaction if, at the completion of the action sequence, the state of affairs then presented does not permit the occurrence of the complete reaction of which the anticipatory reaction is a constituent.
>
> XII: Organisms capable of acquiring anticipatory goal reactions will strive to bring about situations which are reinforcing (7, pp. 24–27).

These three theorems which represent elaborations of the functions ascribed to r_G, s_G (and indirectly, S_d) in the informal account, are derived from a set of six postulates and eighteen definitions (not all of which are used in the derivations). Inspection of the postulate set indicates that it may be taken as an implicit definition of associative learning. Nevertheless it will be observed that all three theorems state fundamental principles of motivation. With regard to theorem XII Hull points out: "An additional element of interest . . . is the fact that the fundamental phenomena of motivation seem to have been derived from the ordinary principles of association. If this deduction should prove to be sound, it will have reduced the two basic categories of motivation and learning to one, the latter being primary" (7, p. 14).

An important consideration follows from Hull's derivation of motivation theorems from learning postulates. Suppose we wished to determine the precise consequences of Hull's three motivation theorems. We would throw them into the form of a postulate set, and proceed to derive theorems. Such theorems would presumably describe some of the more specific phenomena of motivation, *e.g.*, the elimination of pure stimulus acts, etc. Call Hull's adaptive behavior system in which the three laws of motivation appear as theorems, L_1, and the system in which they are taken as postulates, L_2.

Our account of explanation now entitles us to say that the postulates of L_2 represent the first order ('empirical law' level) theory of motivation and the postulates of L_1 represent the second order theory of motivation. If the postulates of L_1 and L_2 were really complete,[10] we could interpret the deduction of the postulates of L_2 from L_1 as the unification of two discrete fields of psychology. Since the postulate set of L_1 is an implicit definition of associative learning, we could now understand 'associative learning' as the explanation of motivation. Thus we find a first elementary instance in psychology of the hierarchical ordering of theory characteristic of physics. It is interesting to note that contrary to the customary assumption that any explanation of a behavioral theory must come from physiology, here we have an example of one behavioral theory serving as explanation for another.

Hull's theoretical procedure.—Let us now turn our attention to the general characteristics of Hull's theoretical procedure. It will be recalled that we named Hull's technique of theory-building the 'telescopic' procedure. This procedure was distinguished from the 'interpretive' in that the step of coordinating empirical constructs to the terms of some pre-existent formal system (interpretation) is omitted.

The 'Hullian' postulates.—Hull's postulates were viewed as consisting of *assumed* empirical laws asserting functional relationships between various constructs. These laws may be either of three types: (1) they may already possess a certain probability weight by virtue of previous experimentation, (2) they may be pure assumptions (hypotheses) which have never been submitted to experimental test,[11] (3) they may represent a mixture of (1) and (2), *i.e.*, the gross relations between the constructs may have empirical evidence, while the specific

[10] Obviously, this is a gratuitous assumption. The mere fact that the postulates of L_1 and L_2 are non-quantitative is a sufficient guarantee of their 'incompleteness.' Thus Hull's feeling that if the deduction of postulate XII proves to be sound, motivation is *ipso facto* reduced to learning, turns out to be a trifle over-optimistic.

[11] Sense (2) of 'empirical law' represents a somewhat unorthodox use of the term. A law is usually understood as a well-verified function. If something has the form of a law but has not been submitted to verification, it is usually called an hypothesis. We take the liberty of calling both law proper and 'hypothesis,' 'empirical law,' in order to emphasize their identity of form.

form of the function is hypothesized. An example of a type (1) postulate is the following (Postulate 6 from the adaptive behavior system): "Each reaction of an organism gives rise to a more or less characteristic internal stimulus" (7, p. 17). An instance of a pure hypothesis may be found in postulate X of Hull's rote learning system: "The total aggregate actual excitatory tendency exerted on a syllable as a reaction tendency is, on the average, a constant for all syllables in a given list" (6, p. 503). Postulate 1 of the adaptive behavior system represents a type (3) postulate: "The adequate stimulation of a sense organ institutes a neural reverberation which persists for some time after the stimulus has ceased to act, the absolute amount of reverberation diminishing progressively to zero but at a progressively slower rate" (7, p. 16). The assumption that stimulation of a sense organ sets up a 'neural reverberation' is inferred from (suggested by) the experimental data on Pavlovian 'trace-conditioning' and the delayed reaction. That "the absolute amount of the reverberation diminishes progressively to zero but at a progressively slower rate" is an assumption consistent with the data, but an assumption nevertheless.

Hull's definitions.—We have already noted that, regretably enough, Hull nowhere devotes explicit attention to the question of operational definition. We cited Katsoff's (10) criticism to the effect that Hull fails to define his terms operationally. In a thoroughly literal sense, Katsoff is undoubtedly right: Hull rarely explicitly states the operational criteria (reductive symptoms) guiding the application of his constructs. But, as we have learned from our discussion of r_G and S_d, Hull does employ a device which serves as a kind of short cut to operational definition. This was the practice of employing explicit definitions of such a character that the *definiens* is, in a sense, able to dictate its own operational symptoms. Instead of overtly listing these symptoms, Hull simply presupposes that his audience will be able to infer them from the form of the *definiens*.

All of the definitions that Hull submits in connection with his three published systems are explicit definitions of this

type. Take Definition III of the rote series system for example: "An excitatory tendency as emanating from a stimulus, is a tendency for a reaction to take place more certainly, and in case it does occur, to do so more vigorously, other things being equal, soon after the organism has received said stimulus than at other times" (6, p. 501). This is clearly a rule stating that 'excitatory tendency as emanating from a stimulus' may be substituted for the expressions comprising the remainder of the sentence, and vice versa. Although the *definiens* expression does not explicitly state the extant reduction sentences for 'excitatory tendency,' it is so formulated that one can supply reduction sentences with little difficulty.[12] The *definiens* here may be looked upon as a kind of summary of the types of operations which could govern the use of this term. It derives this summarizing power by virtue of the fact that it consists of terms whose operational meanings are (presumably) well known. Hull is banking on the operational vocabulary of his fellow psychologists when he uses such terms as stimulus, response, organism, etc.

One of the indefinite number of reduction sentences for excitatory tendency might be: "If a dog is presented with a food stimulus (S) at time t_1 (all other stimuli being eliminated), then if the dog salivates (R) at time t_2, S has a certain finite strength of excitatory tendency with respect to R." [13] This example should make apparent the utter ridiculousness of ever expecting explicit statement of the totality of reduction sentences for a term. For it is possible to write a separate reduction sentence introducing 'excitatory tendency' for every stimulus that can evoke a response. Realization of the strict operationist's dream, then, would involve publication of each definition in something like encyclopedia form. Ulti-

[12] There is nothing mysterious about how the *definiens* of an explicit definition can 'dictate' its own operational symptoms. Any *definiens* consisting wholly, or largely, of terms with operational symptoms so firmly attached that they may be presupposed, is capable of such 'dictation.'

[13] This will be recognized as the positive half of a reduction pair. (See The logical character of the motivation concept. I., PSYCHOL. REV., 1941, 48, pp. 15–39.) It would be improper to use the bilateral reduction sentence in this case, for if the dog failed to salivate it would not necessarily indicate the absence of excitatory tendency emanating from S.

mate rigor is not always compatible with ultimate economy; and the most we can expect of any operational definition is the specification of a sufficiently precise summary of the reduction sentences that could be given for the term—should the necessity arise. Such summaries can, of course, be formulated with varying degrees of precision. Hull might do well to make some of his more definite.

Concluding remarks.—Hull's miniature systems are by no means models of theoretical precision. Not only are they fragmentary and 'small,'[14] but they fall short of mature natural science theory in two important respects: (1) they are formulated verbally; (2) they are non-quantitative.

The fact that they are formulated in English makes them easy to follow, but hardly adds to accuracy of definition, and rigor of deduction. The symbols of the natural word languages carry a bewildering diversity of meanings, and one can never be sure that the meaning intended is the meaning taken. More serious than this is the fact that the logical structure of natural word languages is unnecessarily complex and patently obscure. This fact tends to make rigorous deduction in such a medium exceedingly difficult. There is, however, a very simple and determinate language at the disposal of scientific theorists—the language of symbolic logic. Systems such as Hull's would gain much in precision if formulated in terms of this language.

There is no need to point out why quantification is desirable. No amount of formal elaboration, however, will make qualitative data yield quantitative laws. All that Hull can do in this direction is to set up curve-fitting experiments which seek to determine the mathematical forms of the qualitative functions he has assumed.[15]

[14] 'Fragmentary' because of the limited fields that they cover; 'small' in the sense of paucity of deduced theorems. This is not intended as a criticism, for theory is not an all or nothing affair. Adams (1) has attacked Hull on these grounds. It requires a heroic kind of optimism, however, to insist that *all* of psychology be formalized at one fell swoop. On the other hand, the implacable perfectionism that would condemn a first approximation because it is an approximation is inspiring, but self-defeating.

[15] Since these words were written, Hull has published his most ambitious job of systematization to date: 'Mathematico-Deductive Theory of Rote Learning' (9). This theory, consisting of eighteen postulates and sixty-one theorems, represents an

These limitations of Hull's theories only accentuate the vast gap between theory and what currently passes for theory in psychology. For the distance between Hull and strict theory is infinitesmal in comparison with the distance between most psychological theory and Hull. Even if the deductive procedure that Hull so energetically advocates could contribute no more to present-day psychology than the institution of a warmer intimacy between theory and fact, his enthusiasm will have been well invested.

III. Lewin and 'Interpretive' Procedure

We now come to a psychologist who has developed the most elaborate and extensive group of behavioral constructs yet to be found in psychology—Kurt Lewin. And more interesting than this for our purposes, Lewin's theoretical approach is the only representative in psychology of what we have called the 'interpretive' procedure.

One of the most frequent objections to Lewin's system is that, although he claims to put forward a theory, yet few, if any, empirical laws are in evidence.[16] This is to misunderstand the character of Lewin's work. His topological and vector psychology is not theory, but the framework of theory. His primary job has been to provide psychology with the conceptual tools necessary for theory.[17] Even so, it is not quite accurate to say that Lewin's work is totally devoid of empirical hypotheses. The hypothesis he has elaborated in greatest detail is the so-called 'tension-system' theory. Fortunately for us, the theory surrounding this concept comprises the kernel of Lewin's motivation doctrine. We propose, in this section, first to examine Lewin's implicit definition of motivation (tension system theory), and secondly to show how

expansion of his early system (6) on rote learning. It is exempt from the two limitations listed above. Not only is extensive use made of symbolic logic in the formulation of definitions, but the postulates are advanced, and the derivations proceed, in mathematical form. Space forbids the full discussion that this most elaborate extant application of postulational technique to psychology deserves.

[16] *Cf.* Householder, A. S. Review of *Principles of topological psychology* (2).

[17] This has been the primary task of all systems of psychology, but few system-builders have been so critically aware as Lewin of the nature of their task.

the ambiguity in Lewin's logical procedure is dissolved when the procedure is viewed as interpretive.

Tension system and valence.—Basic to Lewin's doctrine of motivation are the constructs of tension-system and valence. As a first approximation, we may say that the concept of tension-system corresponds to the internal (intra-organismic) motivating conditions which determine behavior. It is analogous in functional significance to such constructs as drive, set, libido, determining tendency and, at least partially, to Hull's 's_G.' Valence, on the other hand, corresponds to the external stimulus properties of certain objects in virtue of which they constitute goals of specific 'behaviors'. Valences are localized in the psychobiological environment; needs have their locus in the inner-personal regions of the person.[18] Not only needs but 'quasi-needs'—intentions—are correlated to tension systems.

Although one can say that a given valence implies some want on the part of the person for the goal-object corresponding to the valence, it is not quite accurate to identify want and valence. Since valence is an environmentally defined concept, it is more aptly characterized as the 'demand-value' (*cf.* Tolman, **14**) possessed by a goal with respect to some particular behavior. This demand-value may be either positive (attracting) or negative (repelling). Operationally, a valence might be defined as follows: If person P is placed at any point within a certain region surrounding a certain goal-object (G), then G has positive valence for P, if, and only if,

[18] The psychobiological field (life space) is the "totality of facts which determine the behavior (B) of an individual at a certain moment." The life space includes the person (P) and the environment (E). Thus $B=f(L\,S)=f(P,E)$. Valences are localized in E; tension systems in P. The person is differentiated into a motor-perceptual region and an inner-personal region. The motor-perceptual region functions as a boundary zone between the inner personal regions and the environment. Lewin does "not at all object to interpreting the inner-personal systems as representing certain brain regions," and the "motoric region . . . correlates at least partly with the muscular system" (**12**, p. 101). The spatial frame of reference of the psychobiological field (life space) should not be confused with physical space. It is a special type of non-Euclidian space designed for the purpose of dealing with the spatial relations peculiar to psychology. As a first approximation it was identified with the abstract non-metrical space defined by mathematical topology. But the treatment of dynamic problems, which necessitates definitions of distance and direction, demands that such a space be endowed with certain metrical properties. Lewin has attempted to fulfill this need for a psychological 'geometry' by the development of a so-called hodological space (*cf.* **12**).

P locomotes in the direction of G; if P locomotes in the direction 'non-G' (away from G) G may be said to have negative valence with respect to P.[19] Valence may also be given explicit definition in terms of other 'Lewinian' constructs: "A positive valence corresponds to a force field where all forces are directed toward the same region"; "A negative valence corresponds to a force field where all forces have the direction away from the same region" (**12**, p. 90). A force field corresponding to a positive valence is called a positive central field; the force field corresponding to a negative valence is called a negative central field.

The most general characterization of tension is that "it is a state of a system S which tries to change itself in such a way that it becomes equal with the state of the surrounding systems" (**12**, p. 98). The tension system concept gains its fundamental motivational significance from the fact that an unfulfilled need or quasi-need corresponds to a state of tension within a system.

The place of 'tension' in Lewinian motivation theory.— Tension systems determine behavior only indirectly through valences and forces. The valence (Va) which an object or activity possesses for a given person is a function of the tension (t) corresponding to the object or activity in question, and the perceived nature of the object or activity (G).[20] In symbols:

$$Va\,(G) = F\,(t,G)$$

But, as we have already seen, "the existence of a valence is equivalent to the existence of a force field" (**12**, p. 90). And behavior in the direction of any goal (G) is always directly determined by some force acting on the person in the direction of the goal ($f_{P,G}$). The force $f_{P,G}$, however, is not

[19] This will be recognized as the verbal formulation of a bilateral reduction sentence. 'Locomotion,' of course, is not to be confused with physical movement. It may be quasi-physical, quasi-social or quasi-conceptual (*cf.* **11**).

[20] 'G', or the 'perceived nature' of the goal-object, depends upon 'certain non-psychological alien factors.' As Lewin points out (**12**, p. 107), "Whether within the environment food exists, what type of food, and at what place is not a result, or at least not alone the result of psychological factors." The writer's best interpretation is that 'alien factors' corresponds to the physical stimulus aspects of the environment.

uniquely determined by the valence $Va(G)$, but is also a function of other factors such as the distance of the person from the goal ($e_{P,G}$). That is:

$$f_{P,G} = F\left(\frac{Va\ (G)}{e_{P,G}}\right)$$

Combining this with the formula for valence, we get:

$$f_{P,G} = F\left(\frac{t}{e_{P,G}}, G\right)$$

This equation reads: "the force for a locomotion depends on the need or tension of the person; on the non-psychological factors which affect the existence of the valence; and on the relative position of the person and the valence" (**12**, p. 107). This formula will serve to define for us the functional place of the tension construct in the derivation of behavior.

In line with our previous conclusion that any implicit definition of motivation cannot consist exclusively of motivation constructs, we may note the presence of G and $e_{P,G}$ in the above equation. Both of these are non-motivational constructs. G introduces a cognitive or perceptual factor. The construct of distance ($e_{P,G}$) refers, of course, not to *physical* but to psychological or perceived distance. This also introduces a cognitive factor. And with cognition all of the constructs which Lewin has introduced in connection with his topological psychology come into the picture. The constructs of force, tension, and valence, on the other hand, presuppose the elaborate array of dynamic or vector constructs that Lewin has developed. Completion of the picture requires the immense scaffolding of undiscovered laws relating these constructs. Thus, final implicit definition of motivation will appear when a complete postulate set for Lewin's psychology appears.

Formalization of tension-system theory.—The fact that psychology is not ready for complete implicit definition of any of her constructs is no reason for keeping postulational technique locked in the cellar in the meantime. There is always *some* hypothesis lurking about, the consequences of which it might

be desirable to explore, and some form of deductive procedure is the only known method for conducting such exploration efficiently.

Lewin, however, has always been a trifle wary about premature formalization of theory. On the other hand he has been promiscuous in his promises of ultimate formalization, and persistent in his claims as to the amenability of his concepts to rigorous formal development. It might, therefore, be instructive to look into the only Lewinian empirical theory which has thus far acquired deductive characteristics.

The theory of tension systems has been verging on formalization for some time now. In *The Measurement of Forces* (12) Lewin submits the following definition of the conceptual properties of tension:

> The concept of tension . . . refers to a *state* of a system of an individual. Tension has besides others the following properties: (a) It is a state of a system S which tries to change itself in such a way that it becomes equal to the state of the surrounding systems. (b) It involves forces at the boundary of the system S in tension (12, p. 98).

The coordinating definition of tension is as follows: "Whenever a psychological need exists, a system in a state of tension exists within the individual." This hypothesis includes also quasi-needs resulting from intentions (12, p. 29).

Lewin then lists as deductions from the conceptual properties given above the following:

> (1) Tendency to resumption of interrupted tasks . . . ;
> (2) A tendency of a tension system to spread over the whole person, especially if the tension is high and if the individual shows a relatively great fluidity . . . ;
> (3) Communication between two systems as a condition of substitute value. The communication is facilitated by specific content and type of transition . . . ;
> (4) Breaking of the walls of the system and release of the tension by sudden changes of the general tension within the person . . . ;
> (5) Diffuse release of tension in time . . . ;
> (6) Dependence of velocity of diffuse release upon the fluidity of the system . . . (12, pp. 99–100).[21]

[21] All of these theorems have been experimentally verified by Lewin's students.

Although Lewin claims that the above represent derivations, the actual derivation of these propositions is nowhere to be found. Now if expedience dictates the listing of theorems without going through the laborious details of their derivation, there can be no quarrel about this practice. But if the claim is advanced that certain propositions are derivations, it is only fair to expect full and specific statement of the postulates from which the derivations have been made. If Lewin's words are taken at face value one gets the impression that these derivations have been made from the two conceptual properties of tension which he lists. But even cursory inspection will disclose that the two postulates *alone* could not possibly generate the above theorems.

This suspicion is confirmed by a recent paper (**13**) in which Lewin actually follows through the derivation of a number of theorems similar to those quoted above. The following are among the additional assumptions used in the derivations:

> The tension $T(S^G)$ is released if the goal G is reached (**13**, p. 18).
>
> To a need for G corresponds a force . . . acting upon the person and causing a tendency of locomotion toward G (**13**, p. 18).
>
> A need leads not only to a tendency of actual locomoting towards the goal region but also to thinking about this type of activity . . . (**13**, p. 19).
>
> Strong waves of emotional tension destroy tension differences corresponding to relatively superficial needs (**13**, p. 23).

By the aid of such additional assumptions Lewin is able to deduce theorems similar to 1, 2, 4, and 5, among others.

Lewin's derivations, in this paper, are comparable in rigor and form to Hull's derivations in his miniature systems. He fails, however, to distinguish sufficiently between postulates, explicit definitions, coordinating definitions and operational definitions. Every proposition employed in his deductions is indiscriminately labeled as an 'assumption.'

This outline of one of Lewin's few extant empirical theories is bound to be a bit confusing without some general orientation in his logical procedure. What, for instance, does Lewin mean by conceptual properties, by coordinating definition? What is the place of operational definition in his procedure?

The precise character of Lewin's procedure is rarely made as clear as it might be in his writings. This may be a result of the economy of his style, or evidence of a flattering faith in the sophistication of his readers. Whatever its cause, however, it is desirable that something be done about it. For the ambiguity surrounding Lewinian methodology has led to many misinterpretations of his system. In the remaining pages of this paper we will attempt to throw the general outlines of Lewin's theoretical procedure into a more recognizable form.

Lewin's technique of theory building.—The claim that Lewin's logical procedure is best interpreted as an instance of the interpretive approach has already become something like a refrain in this paper. We have stated that interpretive procedure is primarily distinguished by the fact that the postulate set of some pre-existent formal system is interpreted (transformed into an empirical system) by the correlation of empirical constructs to the formal terms appearing in the formal postulate set. Such correlation of empirical to formal constructs, we called coordinating definition. The empirical constructs, of course, are introduced by operational definition.

As is well known, Lewin has developed two sets of constructs both of which are necessary for the explanation of behavior. One set is relevant to a topological psychology, the other to a vector psychology.[22] Topological psychology deals with geometrical problems and vector psychology with dynamic problems.

Lewin's topological psychology may be viewed as an empirical interpretation of the already developed formal system of mathematical topology (or a sub-system of this). He correlates to the constructs, appearing in the postulates of the system, certain constructs of empirical psychology. For instance the topological construct of 'path' is coordinated to the psychological construct, 'locomotion' (**11**, p. 92); to psychological construct 'part of life-space' is coordinated topological

[22] The topological constructs are elaborated in *Principles of Topological Psychology* (**11**). The beginnings of the vector psychology are put forward in *The Conceptual Representation and the Measurement of Psychological Forces* (**12**).

'region' (*loc. cit.*). Lewin fails to state, however, the particular postulates of topology which he is interpreting by means of these coordinating definitions. This is understandable enough, for Lewin's book is addressed to an audience not likely to be particularly responsive to the postulates of topology. He therefore finds it expedient to abstract from the postulates the properties with which they implicitly endow the constructs, instead of fully stating the postulates. Householder (**2**) has criticized Lewin for not employing 'a single' principle or theorem from mathematical topology. It should be clear, however, that the fact that *Principles of Topological Psychology* does not look like a topology text-book does not imply that Lewin makes no use of topological principles.

Another and perhaps more fundamental reason for Lewin not stating the particular topological postulates 'into' which he performs his coordinations, derives from the consideration previously noted that interpretation of a formal system usually involves extensive modification of its postulates. Now, although topology appears to be an excellent conceptual tool for Lewin's purposes, there is little point at this stage in 'freezing' his interpretation in a final formulation of a topological psychology. The more cautious procedure, in advancing a first approximation, is to abstract out of the principles of topology those properties which seem most fruitful for the purposes at hand. This is what Lewin appears to have done in connection with the coordinations made in *Principles of Topological Psychology*.

But now we come to a somewhat hidden aspect of Lewinian procedure. Few would suspect that Lewin's vector psychology is undergoing construction along interpretive lines. But certain rather obscure points in Lewin are clarified if we proceed on this assumption.

Lewin has an apparently inexplicable fondness for the term 'dynamics' which he almost incessantly uses, and sometimes in a rather peculiar sense. Considerable light is thrown on this by certain veiled references that Lewin makes to a 'logic of dynamics.' For instance, we find in a footnote to *Principles of Topological Psychology* (**11**, p. 22):

Logically, it may be conceivable that in representing psychological dynamics we can use any kind of concepts. (In other words one may disregard the possibility that there is something like a 'logic of dynamics.') Practically, however, form and content of a theory are both fundamental and we mean to stress both aspects equally. For, a system which is inadequate or useless as to the content of its dynamical concepts will soon prove to be unsatisfactory logically.

In other words, although it is *logically* possible to disregard a logic of dynamics, it is *practically* fruitful to proceed with its aid.

The 'logic of dynamics' is evidently a highly abstract formal system defining the structure of all constructs (*e.g.*, force) pertaining to 'conditions of change.'[23] It is apparently a formal system of such a character that its interpretation by appropriate empirical constructs from any science, will yield the dynamical laws for that science. The dynamical constructs and laws of physics, for example, may be viewed as one such interpretation (perhaps the only fully developed one). It is our belief that Lewin looks upon his vector and dynamic constructs as the psychological interpretation of the logic of dynamics. Thus his coordination of 'need' to 'tension system' may be understood as the coordination of an empirical construct (need) to a formal construct (tension system) *defined by the logic of dynamics*.

This assumption would seem to clarify Lewin's use of 'conceptual properties'. The conceptual properties of the topological constructs are presumably those properties imparted to them by the postulates and theorems of a psychologically interpreted topology; the conceptual properties of the vector constructs are those properties attributed to them by a psychologically interpreted logic of dynamics. Since the interpretation of topology has not as yet attained final form, and since the logic of dynamics is thus far closeted in Lewin's mind, these properties actually state the guiding

[23] This characterization of the logic of dynamics is pieced together by the writer from (1) simple inference from the words, 'logic of dynamics,' (2) Lewin's definition of 'dynamics' (11, p. 213) and (3) certain remarks made by Lewin during lectures. Writer, of course, takes full responsibility.

notions that Lewin *would* use in carrying through the interpretation of both systems. And it is in virtue of this that conceptual properties take on such a fundamental importance for Lewin. We will return to this topic, shortly.

Before continuing further we should note an interesting but unhelpful property of the logic of dynamics—its lack of existence. There are undeniable advantages in proceeding on an interpretive basis, but sheer love of the procedure will hardly add to its fruits when there is nothing to interpret. There is no objection to Lewin's constructing his dynamic psychology along interpretive lines; he may have excellent reasons for building in this fashion. But he must accept the responsibility of making known that which he is interpreting. Lewin must either state the postulates and theorems of his logic of dynamics, or describe its properties more specifically. Either of these alternatives presupposes that first Lewin set up his logic. There is another, and very simple alternative. Lewin could forget about his logic of dynamics and proceed telescopically à la Hull. The chief recommendation for this solution is its economy.

Lewin on definition.—In *The Measurement of Forces* (12, p. 98) Lewin says: "Every dynamical construct in psychology needs a definition of its conceptual properties and a coordinating definition." This apparently represents a minimum requirement for the respectable introduction of a psychological construct. Let us try to discover what is involved in these two types of definition, and determine how they may be brought into integration with the four classes of definition that we have distinguished.

Conceptual definition.—We have already seen that conceptual definition, or definition in terms of conceptual properties, consists in the specification of the properties which the sentences of a psychologically interpreted topology and psychologically interpreted dynamics attribute to the constructs which they relate. Strict conceptual definition corresponds to what we have called (1) explicit definition, and (2) implicit definition. For instance the conceptual properties of 'force' (12, p. 83) are 'direction,' 'strength' and 'point of applica-

tion.' This may be understood as an *explicit* definition of 'force' of the form: an x having direction, strength and point of application may be substituted for any occurrence of 'force' in the sentences of the system of psychological dynamics. An example of conceptual definition taking the form of *implicit* definition might be the two conceptual properties that Lewin assigns to tension system. One cannot, however, be sure that any of Lewin's definitions are implicit, for we have identified implicit definition with definition in terms of a set or sub-set of postulates for a given system. Since it is impossible to distinguish postulates from theorems in either the topological or the dynamic psychology as yet, it may be stretching matters to use the term 'implicit definition' at all with reference to Lewin's work. Nevertheless, even if after formalization the particular sentences specifying the conceptual properties of a construct turn out to be theorems, the fact that the postulates of the system are capable of generating (*i.e.*, contain) these sentences tends to legitimize this usage of 'implicit definition.' And it is clear that in the only aspect of Lewinian theory that has thus far attained formalization—the tension system theory—the conceptual properties given for the definition of tension function as postulates.[24]

Coordinating definition.—In an early section of this paper we noted that Lewin's definition of coordinating definition does not quite correspond to our own. In the glossary to *Principles of Topological Psychology* (**11**, p. 213) he defines coordinating definition as follows: "certain observable facts are correlated to certain mathematical concepts." It will readily be seen that this characterization is a hybrid consisting of properties which we have discriminated into 'operational' and 'coordinating' components. On our account 'certain observable facts' (reductive symptoms) are *first* correlated to empirical constructs by operational definition, and the now defined empirical constructs are *then* correlated to 'mathematical concepts' (formal terms), through coordinating defi-

[24] The tension-system theory may be viewed as a fragmentary sub-system of what will some day blossom into the dynamic psychology.

nitions. Thus, if we insert the missing link of empirical construct into Lewin's formula, his coordinating definition bifurcates into 'operational' and 'coordinating' elements.

Lewin's actual working illustrations of coordinating definitions are almost invariably coordinations of our type. The following are typical Lewinian coordinating definitions: "To each part of the life space a region is to be coordinated" (**11**, p. 93); "Whenever a psychological need exists, a system in a state of tension exists within the individual" (**12**, p. 99). It will be noticed that in each case a *psychological construct* (not an observable fact or a reductive symptom) is coordinated to a formal construct.

Operational definition.—If Lewin's coordinating definitions are uniformly coordinations in our sense, where do his operational definitions enter the scene? Clearly, if he fails to provide operational definitions for the empirical constructs which he employs, no amount of coordination will give his constructs empirical meaning. At least one of his critics (**12**) has already made the standard complaint of failure to satisfy the rigors of operational definition.

The answer to this kind of question is simply that Lewin is no less and no more rigorous with regard to the operational definition of his terms than any of his professional colleagues who write meaningfully. His practice is the customary one of either assuming that the reductive symptoms for the empirical constructs which he uses are known by his audience, or informally indicating some of the operations which define his terms in the general discussion surrounding their introduction. For instance, after introducing the coordinating definition of need to tension, Lewin says: "The empirical facts to which this statement refers, are those acts of behavior which generally are recognized as a syndrome indicating need" (**12**, p. 99). Despite the fact that this procedure is perfectly permissible, one might still wish that Lewin explicitly provide operational definitions of his crucial constructs. There is no question about the fact that all of Lewin's constructs are operationally *definable*. But there is a distinction between a

definable construct and a *defined* one: in the latter case the writer can fix his meaning to any degree of precision desired, in the former the reader is presented with several degrees of freedom for misinterpretation.

Returning now, to Lewin's treatment of operational definition in his methodological discussions, we find that in *The Measurement of Forces* he uses 'operational' and 'coordinating' definitions synonymously. He describes both operational and coordinating definitions as follows: "An empirical process or operation is defined which permits one to determine whether or not in a concrete case the dynamical fact exists." This, of course, corresponds to operational definition in the ordinary sense. In spite of this identification, the coordinating definitions which Lewin advances in the same work correspond consistently to our sense of the term (*i.e.*, they correlate 'empirical' to formal constructs). For example, the formal construct 'resultant of forces' is correlated to the empirical construct 'locomotion in the direction of the resultant,' or to a 'restructuring' of the field (**12**, p. 85), and, as we have seen, 'tension' is correlated to 'need.'

Lewin's procedure would gain much in clarity if he made a sharp distinction between operational and coordinating definition. For one thing he would achieve closer harmony between what he does, and what he says he does. For another it would tend to make the interpretive character of his constructions more explicit. Finally it would reduce the probability of his readers (and critics) becoming confused about the place of operational definition in his procedure.

The ambiguities that we have noted in Lewin's theoretical methodology have not been singled out with destructive intentions. On the contrary, the stringency of our demand for the clarification of certain points proceeds from a conviction that Lewin's constructs are of unparalleled importance for psychology. It would be a pity if the work of one of the most advanced theoreticians in the science should be misunderstood because of a few misleading statements, an occasional discrepancy between word and deed.

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THE FUNCTIONING OF KNOWLEDGE OF RESULTS IN THORNDIKE'S LINE-DRAWING EXPERIMENT[1]

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In his book *Human Learning*[2] Professor Thorndike reports a line drawing experiment as follows:

> You sit at your desk with a large pad of paper and a pencil, close your eyes, say, "Draw a four-inch line with one quick movement," and again and again draw with one quick shove a line intended to be four inches long. You keep your eyes closed throughout. Day after day you do this until you have drawn 3,000 lines, no one of which you have ever seen (p. 8).
>
> Table 1 shows the results of such an experiment. It illustrates two general truths or principles: (1) that of multiple response or variable reaction, and (2) that of the failure of repetition of the situation to cause learning (p. 10).
>
> Repetition of the situation 3000 times caused no learning. The lines drawn in the eleventh and twelfth sittings are not demonstrably better than or different from those drawn in the first and second (p. 10).
>
> Our question is whether the mere repetition of a situation in and of itself causes learning, and in particular whether the more frequent connections tend, just because they are more frequent, to wax in strength at the expense of the less frequent. Our answer is *No* (p. 13).
>
> The repetition of a situation may change a man as little as the repetition of a message over a wire changes a wire. In and of itself, it may teach him as little as the message teaches the switchboard. In particular, the more frequent connections are not selected by their greater frequency (p. 14).

The further analysis of Thorndike's data which will be given in this paper was instigated by the sentence, "The lines drawn in the eleventh and twelfth sittings are not

[1] This paper was developed incidentally to a larger study of motor abilities, subsidized in part by the National Research Council.

[2] E. L. Thorndike, *Human learning*, New York: Century Co., 1931. Pp. 200.

TABLE I [3]

DISTRIBUTION OF THE RESPONSES AT EACH SITTING IN DRAWING LINES TO EQUAL 4 INCHES WITH EYES CLOSED: SUBJECT *T*

Response	1	2	3	4	5	6	7	8	9	10	11	12
3.7									1			
3.8								2				
3.9												
4.0			3						3			
4.1			4	1				1	3			2
4.2		4	8			1		3	6	1	2	1
4.3		3	9	1				4	5	3		4
4.4		13	12	6			3	4	12	2	4	3
4.5	3	18	18	14	2	7	3	15	14	8	7	11
4.6		20	23	23	3	7	8	13	14	8	14	11
4.7	6	20	14	22	11	14	16	25	13	9	14	21
4.8	6	22	15	18	14	27	17	16	18	15	19	26
4.9	13	17	24	24	22	28	18	21	16	10	18	30
5.0	25	20	16	24	26	21	29	25	14	24	19	20
5.1	27	10	16	12	25	32	14	15	14	22	31	22
5.2	24	11	8	12	24	21	23	25	16	18	28	16
5.3	30	8	2	11	21	13	17	8	18	18	16	12
5.4	17	4	2	8	10	10	7	8	12	12	7	7
5.5	12	1		4	13	8	7	3	10	13	4	3
5.6	7			2	4	7	4	1	4	5	2	2
5.7	3			1	4	2	5	2	6	4	3	1
5.8					1			1		2		
5.9	1				1					1	2	
6.0												
6.1												
6.2	1						1					
Total	175	171	174	183	181	198	172	192	200	175	190	192
Median	5.23	4.83	4.77	4.93	5.15	5.07	5.07	4.96	4.97	5.13	5.09	4.96
Q [4]	.16	.22	.23	.22	.19	.19	.21	.24	.33	.24	.21	.20

demonstrably better than or *different from* those drawn in the first and second," and by the first sentence in the last quoted excerpt. The writers will demonstrate that there is a progressive change in behavior of Subject *T* which can be determined from data in Table 1 and will further argue that a satisfactory explanation must be inconsistent with the statement, "The repetition of a situation may change a man as little as the repetition of a message over a wire changes a wire." According to the mass of our psychological and physiological principles a subject who draws about 3000 lines under

[3] This is Thorndike's Table I as found on page 9 of *Human Learning*.

[4] *Q* is the half of the range required to include the middle 50 per cent of the responses.

conditions of high motivation to make them just four inches long *will be different* as the experiences multiply. The wire analogy is overdrawn and dangerous.

The writers have no disagreement with the whole of Thorndike's data and argument as presented in *Human Learning* and in more experimental detail in the *Fundamentals of Learning*.[5] Thorndike set out to establish the relationship between repetition of a situation and the waxing and waning of those 'connections' which appeared early in the learning process. The broad conclusion given in the fourth quoted excerpt is obviously sustained by his research. Further, the writers are supposing that the idea they are about to elaborate is not unknown to Thorndike; his research program did not require this further interpretation of the data. The ideas in the two sentences to which the writers take exception do not seem to have been raised in the extended presentation in the *Fundamentals of Learning*, although Thorndike is inclined to believe the shifts in central tendency from sitting to sitting are essentially chaotic.[6]

Since the sentences were made in relation to a set of data on Subject *T*, one can assume that Thorndike considered this performance a prototype of data from several subjects. The analysis in this paper is of Subject *T*. In an appendix similar analyses, with brief notes, are made of data from two other subjects.

The principle that knowledge of results facilitates learning has been demonstrated too often to need further discussion here. This principle implies, however, that various amounts of knowledge of results must lead to various degrees of learning, at least when the learner is well-motivated and recognizes his goal. If Subject *T* in Thorndike's line-drawing experiment was an alert and reasonably well-motivated person, the experimental situation as described above can be regarded as one in which Subject *T* had some kind and degree of knowledge of results. Obviously *T* did not know the measured

[5] E. L. Thorndike, *Fundamentals of learning*, New York: Bureau of Publications, Teachers College, 1932. Pp. 638.

[6] *Fundamentals of Learning*, p. 501.

lengths of the lines he was drawing, but the data in the table verify the common-sense observation that *T* must have been aware of the fact that he was not drawing lines twelve inches long or one inch short. If we assume that *T* did have some knowledge of results, then analysis of the data should reveal some trend toward modification of *T*'s behavior during the 2203 lines drawn by *T* during the twelve sittings. Contrary to Thorndike's statement, the lines drawn during the eleventh and twelfth sittings should be demonstrably 'different from' the lines drawn in the first and second sittings. If the demonstrated difference is to be considered as learning it must also have the characteristic of being an orderly trend.

These observations may be stated as propositions:

a. Knowledge of results facilitates learning.

b. Subject *T* had some knowledge of results.

c. The knowledge of results which *T* had and his currently increasing body of 'past experience' must have modified his responses.

d. The modification of responses would have to be in terms of the knowledge available to *T*. (In other words, the lines drawn would not necessarily tend to change in terms of an objective four inches, but in terms of the experiences of line-length which *T* was having.)

If these four propositions are correct, Table I should reveal a tendency for succeeding lines to vary less and less from the central tendency of the lines previously drawn. The following discussion and data are the result of an analysis of Thorndike's table to discover this predicted modification of behavior.

1. *T*'s concept of a four-inch line, in terms of his behavior, was not a standard four inches; all we know is that at the end of his 2203 trials his mean performance was 5.01 inches. This may be defined as *T*'s "experiential 'four-inch' line." Therefore, evidences of learning will have to be found in the modification of behavior which both depends upon and determines this personal norm.

2. Following is a table of the means for each sitting, the standard deviation of each mean, the deviation of each mean

from the mean of the total (5.01), and the deviation of each median from the median of the total (5.02).

TABLE II

Means	5.23	4.85	4.76	4.94	5.16	5.08	5.10	4.95	4.97	5.12	5.05	4.96
Sigma	.26	.29	.31	.30	.27	.28	.34	.34	.43	.34	.31	.29
Deviation of means	.22	.16	.25	.07	.15	.07	.09	.06	.04	.11	.04	.05
Deviation of medians	.21	.23	.25	.09	.13	.05	.05	.06	.05	.11	.07	.06

These data indicate an increasing stability of performance from sitting to sitting when the average of all the responses is considered as the reference point. This does not mean that the subject had a conscious goal of 5.01 inches as his concept of a 'four-inch line.'

Strictly speaking, the increase in stability which this analysis suggests can only be explained as the effect of preceding trials on succeeding trials. Therefore, a different type of calculation was made.

3. When *T* drew the *first* of his 2203 lines, he must have been governed by his *then existing* concept of a four-inch line. Every succeeding line-drawing act not only progressively changed his concept of a four-inch line, but was in part determined by the cumulative effect of his preceding experiences. If this is so, the mean and median of each sitting from 2 to 12 should deviate less and less from the mean and median, respectively, of the total number of lines drawn previously to that sitting. The results of this analysis are given in Table III.

TABLE III

		Means	Medians
Dev. of sitting 2 from sitting	1	.38	.49
3	1– 2	.28	.19
4	1– 3	.01	.04
5	1– 4	.21	.19
6	1– 5	.09	.06
7	1– 6	.09	.05
8	1– 7	.07	.07
9	1– 8	.04	.05
10	1– 9	.12	.12
11	1–10	.03	.06
12	1–11	.05	.07

4. Let us now divide the 12 sittings into 4 groups of 3 sittings each: group 1 (sittings 1, 2, 3) is an early stage of *T*'s concept formation, while group 4 (sittings 10, 11, 12) is a later stage of concept formation. If *T* is forming an increasingly stable concept, however factually erroneous, the three sittings *within* group 4 should vary less from each other than those in group 1. The average length of all the lines in sittings 1, 2, 3 was calculated and the mean deviations of the three sittings, from that figure, were in turn averaged; similar computations were made for all the groups. The data are in Table IV.

TABLE IV

	Average Deviation of the Three Means
Group 1 (1, 2, 3)	.216
Group 2 (4, 5, 6)	.080
Group 3 (7, 8, 9)	.070
Group 4 (10, 11, 12)	.050

This evidence presented indicates that *T*'s responses were modified. These modifications were not random but systematic in character and can, therefore, be regarded as evidences of learning. This learning occurred because *T* had knowledge of results.

While Thorndike was correct in concluding that under the given experimental conditions the subject would not improve his performance in terms of an objective four inches and that repetition would not in and of itself fixate the initially predominant line-lengths, he was not correct in concluding that ". . . the lines drawn in the eleventh and twelfth sittings are not demonstrably better than or different from those drawn in the first and second." To be sure, they were not *better* in terms of four inches on a ruler, but that they were *different* it is impossible to deny.

DISCUSSION

This paper was begun because of a statement by Thorndike which does not seem to emerge from the data given to support

it. The faulty statement, however, is not of crucial significance in the main line of argument in *Human Learning* and *Fundamentals of Learning*. Through this further analysis of the data attention is called to a need for clarification of the role of knowledge of results in learning.

The writers feel that in the line-drawing experiment, as in other situations in which conscious, motivated subjects are placed for observation of their trends of behavior, we have no basis for asserting or assuming that the subject has no knowledge of results and therefore learning will not occur. A conscious, motivated subject must know something about what he is doing. Even erroneous conceptions of what is occurring in his performances constitute part of his knowledge of results. The accuracy of his private body of knowledge of results depends upon his previous experiences in similar, or even quite remote, situations and upon the sensory cues which are now available to him. Remembered experiences (ideational or motor) and current sensory data, operating in an alert, well-motivated person, are variables which should enable him to learn, even though he has no objective evidence from other persons or by self-measurement with which to check himself.

Ordinarily, as in classrooms, we include under the caption 'knowledge of results' only the external applications by other persons of verbal or physical approval or disapproval, with or without details of the successes and failures. Sometimes we permit the learner to gain knowledge of results by self-measurement; we give him physical helps such as rulers, answer books, etc. It is common practice in learning experiments to assert that knowledge of results is not a factor unless introduced in either of these ways. Our main interest, as experimental educational psychologists, in the effects of tutelage, guidance, and knowledge of correct solutions on learning in everyday and school situations has caused us to neglect the study of more subtle knowledge.

However, learning theory must consider other sources of knowledge of results even if these minor sources lead to little learning or even to learning of faulty responses. The psy-

chological analysis of the formation of concepts and of the establishment of motor habits requires that we inspect what the subject himself contributes in the way of analyzing his own performance. The contributions to learning which come from these cues and the interpretations of them by alert, motivated subjects may not always be great; on the other hand they may be considerable. Explanations of transfer of training, positive and negative, must include the variables here indicated. The more passive the learner the less his personally derived knowledge of results will be; the more his past experience is applicable and the more he agressively seeks to interpret the multitude of sensory cues currently available, the greater the role of this sort of knowledge of results.

The foregoing argument does not demand that performances increase in goodness. It only asserts that, given some knowledge of results, faulty or accurate, derived either from objective, external sources or from internal manipulation of memories and current stimulations of a limited nature, there will be changes in behavior which tend to become stabilized.[7] Individual differences in alertness, motivation, past experience, and sensory acuity will be observed.[8]

Experiments to analyze more precisely the functioning of these lesser sources of knowledge of results should not be too difficult to plan and should contribute to a general theory of learning. The writers have done a preliminary study with only a few subjects which indicates differentials in learning in groups with different degrees of knowledge of results ranging all the way from passive guidance of the subjects through the act to be learned to the conventional reporting of measured error to the subject.

[7] Some of this stabilization may be in other aspects of the situation than the precise task being studied. Thorndike (*Fundamentals of Learning*, p. 14) mentions some such changes. Doubtless emotional reactions to the whole situation change too.

[8] While the foregoing analysis has indicated an increasing stability of performance from sitting to sitting, it also shows that the standard deviations, as well as the interquartile ranges, do not show a tendency to decrease. In agreement with Thorndike (*Fundamentals of Learning*, p. 499) a tentative explanation may be that the measure of variability within each sitting is a measure of *T*'s kinesthetic acuity and motor control.

Appendix

Much original data for further testing of the observations made on Subject *T* are available in Chapter II and Appendix II of the *Fundamentals of Learning*. It is not to be expected that every one of these subjects should show as neat learning curves as are seen in the tables in the main body of this paper. Individual learning curves are usually irregular and some members of a sample may deviate greatly from the trend. Furthermore, the hypothesis regarding non-externally derived knowledge of results which is the topic of this paper might have been functioning minimally in some of the subjects because, as far as the reports indicate, they were not given to understand that any improvement through self-management was expected. This is not to say that they were not motivated to do a respectable job for the experimenter, but the writers can easily imagine simple ways of enhancing the learners' appreciation of the roles of current sensory cues and the importance of establishing a stable concept, muscularly expressed, of a four-inch line.

Two cases, LH, who drew four-inch lines, and WO, who drew six-inch lines, are presented in tables comparable to Tables III and IV in the main text. LH's performance is like Subject T's and is consistent with the hypothesis. WO's is not and was selected as the most extreme case of instability of performance.

Table IIIa

Deviations of Mean of Each Sitting from the Mean of All Lines in Preceding Sittings. Subjects LH and WO.

Deviations of mean of sitting	from mean of 2 sitting(s)		LH–4-inch lines	WO–6-inch lines
2	1		.90	.10
3	1– 2		.07	.06
4	1– 3		.14	.29
5	1– 4		.93	.35
6	1– 5		.74	.44
7	1– 6		.10	.75
8	1– 7		.07	.76
9	1– 8		.33	.21
10	1– 9		.27	.10
11	1–10		.02	.27
12	1–11		.35	.71
13	1–12		.28	—

TABLE IVa

SITTING-BY-SITTING VARIABILITY AT DIFFERENT STAGES IN THE SERIES OF SITTINGS. SUBJECTS LH AND WO

	LH–4-inch lines		WO–6-inch lines	
	Mean of Group	Ave. Dev. of Group	Mean of Group	Ave. Dev. of Group
Group 1 (Sittings 1–3)	2.88	.333	5.14	.083
Group 2 (Sittings 4–6)	3.48	.383	5.55	.096
Group 3 (Sittings 7–9)	3.25	.090	6.04	.170
Group 4 [9] (Sittings 10–13)	3.43	.107	5.97	.263

[9] Group 4 is made up of sittings 10–13 for LH and 10–12 for WO.

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SOME ANTECEDENT CONCEPTS OF PERSONALITY TRAIT

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The concept of personality trait was practically unknown before 1900. Since persons thought, spoke, and wrote in terms of soul, faculty, idea, instinct, ability, etc., prior to that date, it is a reasonable inference that personality trait is a vogue in the progression of concepts. It expresses many characteristics of these preceding notions. Contemporary students of personality problems who overlook this fact frequently consider these earlier terms as obsolete, ignore their rich heritage, forget their functional interdependence with particular periods of collective thought, and perceive the unique function of personality trait from a wrong perspective.

Before special similarities in the successive concepts are indicated, some general characteristics will be noted. The following observations are based upon an historical survey of 145 writers [1] whose modes of thought are related to personality trait.

Different concepts predominate in various periods of thought. Soul was fashionable from Plato to Hume. Faculty was prominent until Descartes. After a period of disuse, it reappeared with Wolff and persisted through the writings of Caldwell. Ideas were popularized by Descartes, and they prevailed to Darwin. Evolutionary thought by Darwin and others supplanted ideas. Hereditary elements were emphasized in several ways. Wundt spoke of sensation. James conceived of instinct. Galton referred to ability. The concept of personality trait was introduced by Cattell and Cooley.

The history of a concept can be usually portrayed by the

[1] References to particular names should not be interpreted as pertaining exclusively to the writings of those authors but to their times. Some writers employed several of the concepts that are considered in this article.

Gaussian curve. To illustrate, power was seldom used before Locke. It was commonly employed from Locke to Darwin. Then its usage declined. Again, attitude was rarely mentioned before Binet. Its use increased during the second and third decades of this century. At present, it competes with new terms such as unit of behavior, reaction, behavior pattern, and item of behavior. In all probability, its popularity will wane.

Concepts that dwindle in usage are seldom completely eradicated. For instance, quality, characteristic, and habit were concomitant with soul, faculty, and idea, as well as with attitude, trait, reaction, and unit of behavior. Locke, Berkeley, Hume, and Hartley criticized innate ideas, but the notion reappeared in Kant and later writers. Locke and Herbart attacked the theory of faculties, but it was used by educators as recently as the last quarter of the nineteenth century. Hume denounced the belief in a soul substance, but it was reaffirmed by later writers such as Herbart and Lotze.

In some periods of thought, opposing concepts strive for positions of dominance. For example, innate ideas (Spinoza) and acquired ideas (Locke) were in competition. Likewise, acquired elemental ideas or sensations (Herbart) and organically inherited faculties (Spurzheim) were in conflict. Occasionally, an attacked concept appears in a disguised form in the writings of a critic. Locke denied innate ideas though he accepted innate powers. Herbart rejected faculties yet he posited a multiplicity of elemental ideas.

Ordinarily, concepts do not retain their original meanings. They become disconnected from the primary experiences. Their denotations expand, contract, and sometimes change completely. Soul and faculty exemplify this point. For Aristotle, soul meant an active, universal substance. Vives defined it as the principle of life. Descartes thought that it was material as well as immaterial. Spinoza used it synonymously with consciousness. Locke considered it a spiritual substance. Herbart said that its primary nature was unknown. Wundt included the whole content of psychological experience in his usage of the term. Concerning the concept

of faculty, Aristotle defined it as potentiality. For Albertus Magnus, it signified energy that explained particular acts. According to Aquinas, it was in contrast to the essence of mind. Comenius stated that a faculty was organically inherited and susceptible of training. Leibnitz believed that it was an abstract power. D'Alembert made it a psychogenetic factor. Fries said that it consisted of three stages, namely, sense, habit, and understanding. Galton referred to it as natural or acquired behavior. Roark defined it as a manifestation of mind.

The definitions of personality trait are as multiple as those of soul and faculty. F. H. Allport and G. Allport define personality trait as general, permanent, neurological adjustments. Burgess thinks that the concept refers to the personal behavior patterns that originate in the child's early social experiences and that become permanent social adjustments. Freud considered traits as the resultants of the interaction between the child's original sexual impulses and the repressive actions of family members. Dewey defines the term in reference to group expectations of personal conduct. Hartshorne and May think that the term denotes specific manifestations of behavior. Valentine says that it is a psychological abstraction.

Not only does the meaning of a concept change, but also different concepts refer frequently to similar problems or meanings. In other words, the problems that are embodied by earlier concepts do not always automatically disappear with them. Instead, they are frequently incorporated into later conceptual organizations of thought. It is this aspect of the antecedent concepts to personality trait that most fully reveals the similarities between them, and it will be therefore elaborated in order to provide a factual basis for the deductions that will be made later regarding personality trait. Some of the problems that were considered in the successive concepts of faculty, idea, instinct, and ability are the following: (1) mutability versus immutability of conduct; (2) specificity versus generality of behavior; (3) elemental and complex qualities of human nature; and (4) distribution of charac-

teristics. Today, these are some of the fundamental problems of personality trait. What has been the treatment of these problems in these several conceptual organizations of knowledge?

Regarding the problem of mutability, writers who considered it in terms of faculty disagreed in their answers. Aristotle, Albertus Magnus, and Wolff thought that faculties were permanent and unchangeable. Spurzheim believed that they were organically determined. Shaftesbury accepted the permanency of the moral sense faculty. Comenius ascribed a hereditary permanence to faculties though he allowed for some modifications through experience. Helvetius granted the existence of natural faculties but contended that they were mutable.

The ideas of Locke made obsolete the concept of faculty but the problem of permanence and change remained. Locke admitted the persistence of innate powers and an inner sense of reflection. Likewise, Descartes acknowledged the permanence of innate ideas. Yet, both men defended the rational modifiability of many phases of individual conduct. Hume demonstrated the absence of any permanent identity in oneself. Through pure reason, Kant discovered permanent innate forms and, through practical reason, he found experiential changes in conduct.

The disclosure of the hereditary basis of ideas curtailed their popularity. Ideas yielded to instincts that were organically inherited. Spencer and Hall endorsed permanence and change in their recapitulation theories. F. A. Woods and Davenport maintained that instinctive behavior was permanent except for organic variations. Lombroso and Dugdale championed the thesis that certain pathological behavior patterns were hereditarily determined. Griffitts approved of permanence and change in his maturational theory. Permanence existed in the dormancy of most behavior at birth. Change occurred through the chronological ripening of instincts in the individual's life.

The popularity of instinct was almost concurrent with the prestige of ability. The latter also dealt with permanence

and change. Galton presented evidence of the organic permanence of abilities. Pearson conceded that education could foster some abilities but he was certain that most abilities were bred. Seashore submitted experimental evidence for the constancy of musical ability, and Stern cited other experimental evidence for its changeability. Spearman amassed statistical data that proved the permanence of some abilities and the impermanence of other abilities. Godfrey Thomson gathered similar statistical data but concluded that it neither proved nor disproved Spearman's conclusions.

Among the users of personality trait, there is equal diversity in the answers to this problem. Zoepffel, Washburn, and Paterson affirm the permanency of particular traits. Gordon Allport, F. E. Howard and Patry assert that all traits are permanent. Hollingworth, Kitson, and Brandenburg accept the permanency of some traits and the changeability of other traits. Lowrey, Wickman and Gilliland endorse the modifiability of all traits. Trow, Voelker, and Mary E. Walsh refer to the mutability of particular traits. Gesell and Downey believe that traits are variable within permanent limits.

The second problem pertains to the consistency or inconsistency of an individual's actions. Does an individual's activity in one situation influence his activity in other situations? The generality or specificity of faculties varied according to the number of faculties that were assumed by such men as Zeno, Gall, Tetens, Mendelssohn, and Caldwell. Aristotle regarded the human body as subservient to the soul. Thus, he created theoretically a unity rather than a juxtaposition of faculties in the individual. Unity was also attained through transfer of training. In the field of formal discipline Alexander Hegius considered the learning of Greek as a prerequisite to the understanding of grammar, rhetoric, mathematics, history, and holy scripture. Similar thoughts were expressed by Knox. Spurzheim asserted the independence of faculties from one another. He contended that faculties would not be disproportionately developed if there were a unity in the individual's behavior.

Locke supported the generality and specificity of ideas in his associational theory. Simple ideas which were specific experiences were combined by the mind into complex ideas or general experiences. By bringing either simple or complex ideas together without actually uniting them, there arose ideas of relations, and by separating them from all other ideas, *i.e.*, by abstraction, general ideas were formed. In other passages, Locke referred to some ideas as having a natural correspondence and connection with one another, while other ideas owed their connections wholly to chance or custom. Similar positions were held by J. S. Mill and Hartley. The identity of ideas was discredited by Hume who maintained that common ideas were based upon memory traces. Kant postulated an irreducible number of inner forms or categories that did not arise from experience though they made possible organized experience and unity of self. According to him, the apperceptive judgment, which was originally mentioned by Leibnitz, gave the organization to experience and unification to the self in the process of experiencing. Thus Kant emphasized specificity as well as generality of conduct. Herbart assumed specific elemental ideas which were assimilated into general ideas. In addition, he contended in one passage that no experiences were identical, and in another passage that experiences were united in the apperceptive process and that the soul gave the fundamental unity to all individual activity.

Among instinctivists, generality and specificity were implied in their controversy about the number of instincts. McDougall, Colvin, and Griffitts gave lists of many instincts. W. I. Thomas, Freud, and Trotter mentioned only a few instincts. The former favored specificity and the latter supported generality of behavior.

Münsterberg, Terman, and Kitson referred to the general ability of intelligence. Galton, Seashore, Bronner, and Paterson described collectively the special abilities of music, language, mechanics, vision, and mathematics. Spearman secured test scores that indicated the existence of general ability and specific abilities. His conclusions were endorsed

by Carey, Garnett, Webb, and Burt, but were challenged by Thomson, Hull, and Kelley. Thorndike, who secured only low correlations among various abilities, concluded that they were specific non-focal reactions. Stumpf who worked with tones found that the ability to concentrate, regardless of the field where it was acquired, was efficacious in other activities. Ebert and Meumann demonstrated the transfer of memory but James found no conclusive evidence for such a transfer. Wissler proved by test results that quickness, accuracy, etc., were specific abilities rather than fundamental attributes of all the acts of a person.

Concerning personality traits, Piaget and Royce interpret all behavior in terms of one basic trait. Burgess believes that there is one basic trait in each individual's behavior but he does not limit the variety of basic traits among individuals. Wickman and Bühler restrict the variety of basic traits though each person displays only one. Downey and Floyd Allport mention two or more general traits in the individual. Moore, Gilliland, and Emmons write about the generality of particular traits. Hollingworth, Freeman, and Ezekiel maintain that some traits are general and others are specific. Trow, Hartshorne, and May accept the specificity of all traits. Filter, Landis, and Dowd mention the specificity of particular traits.

The third problem pertains to the complexity of conduct. Are there degrees of complexity? Is conduct reducible to elements? To these and similar questions, answers have been proffered. Comenius prescribed a hierarchy of faculties. The external senses were more elemental than the internal senses. Among the latter, the ascending order of complexity was imagination, memory, and reason. Palmer believed that the development of faculties was from the simple to the complex, paralleling the racial development of mankind. Christian Wolff reduced all complex experiences to elementary faculties. Caldwell accounted for complex behavior through combinations of elemental faculties.

The doctrine of innate ideas posited organically inherited elements of behavior. In addition, Descartes analyzed com-

plex emotions into six elementary emotions. Locke accepted elementary ideas and feelings that were combined into complex ideas and feelings. He was anticipated in this view by Vives and Comenius. Hartley formulated laws of mental association that referred to the development of complex ideas from elemental ideas. Likewise, Herbart assumed elemental sensations and ideas which became complex through blendings, complications, and mutual inhibitions.

Griffitts had a hierarchy of instincts. Elementary instincts were manifested shortly after birth. Complex instincts did not ripen until later years. McDougall thought that instincts and emotions were the elements of complex sentiments. Edman believed that normal behavior was always complicated by the presence of several instincts such as pugnacity, submissiveness, and acquisitiveness which were interwoven with one another. Tansley implies that dominant systems or complexes in individual behavior are composed of elemental, powerful instincts. James regarded instincts as the elements or raw materials of complex habits. In his chapter, 'Stream of Thought,' he rejected the existence of discrete entities.

Thorndike, Valentine, and Seashore assert that abilities, viz., musical, sense of discrimination, memory of digits, acuity of vision, and sense of time, are elemental because they do not vary in test results with training, intelligence, or age after the child is intelligent enough to observe. McDougall refers to intelligence as an inborn, constant, complex ability that includes a variety of special forms such as memory, arithmetical ability, musical capacity, aesthetic taste, etc. Without elucidation, he names tenacity of memory a single or elemental ability, and the other forms complex.

Elizabeth Morris, Dougherty, and Moss acknowledge the complexity of personality traits in their construction of tests. They enumerate components of a trait, list degrees of a trait, submit a battery of tests, etc., all of which suggest more elemental aspects of the complex trait. Rugg, Filter, and Zyve ascertain the precise relations between a trait and its constituent parts which is tantamount to a recognition of

complex and elemental traits. Morton Prince defined some or all traits as complex, as do also Gordon Allport and Margaret Curti. In the interpretation of measurement results, W. Brown, Cleeton, and Truman Kelley state that the tests of selected traits are simultaneously partial measures of other traits which is an endorsement of the complexity of conduct. Charters and Tucker use as a criterion of an elemental trait a change in meaning when it is separated into parts.

The fourth problem deals with the distribution of characteristics. Are the qualities between lower animals and human beings, primitive and civilized, child and adult, male and female, normal and abnormal, distributed quantitatively or qualitatively? Aristotle implied a continuous distribution of faculties in his evolutionary scheme. Gall referred to the qualitative differences in the faculties of human beings that were due to unequal organic inheritances. Spurzheim maintained that there were qualitative and quantitative differences in human faculties. Lavater thought that the characteristics between men and women were frequently different in kind. In the common attributes between the sexes, those of the women were usually inferior.

Although Descartes made no direct reference to the distribution of ideas, he designated the soul and reason as unique characteristics of human beings. Lower animals were virtually mechanical automatons. La Mettrie questioned this position in his description of the similarities between man and lower animals. Rousseau asserted that there were marked qualitative differences in feelings and ideas between the child and adult worlds. He anticipated the viewpoint of Piaget.

Herbert Spencer accepted the continuous distribution of characteristics between man and lower animals though he thought that there were qualitative differences between the civilized adult and the primitive or child. James maintained that man had more instincts than animals which indicated qualitative differences. Davenport mentions qualitative differences between the normal adult and the infant, feebleminded, or criminal. Griffitts accounts for the diverse vocational choices of persons in various age periods by the pre-

dominance of qualitatively different instincts. Stanley Hall, Paul Popenoe and R. H. Johnson account for racial dissimilarities by qualitative differences in instincts and organic inheritances.

Galton approved of the Gaussian distribution of abilities. Ribot confirmed this viewpoint as it applied to the differences between normal and abnormal persons. In early writings, Binet maintained that the abilities of the child and adult were qualitatively different. In later writings on intelligence tests, he abandoned this position. Thorndike, Terman, Stern, Pearson, and Spearman accept the bell curve distribution of abilities. Kent, Rosanoff, and Cattell think that there are qualitative and quantitative differences in abilities. From test results, Murdoch concluded that there were qualitative differences in the mental abilities of racial and national groups.

Olson, Viteles, Dragoo, and Cornell endorse the Gaussian distribution of personality traits in an unselected, random group. Ogburn, Humm, and Wadsworth extend this viewpoint to the differences between normal and abnormal persons. Hull expounds a theory of 'trait differences' that refers with approval to the Gaussian distribution of traits within an individual. Both F. H. Allport and G. Allport consider some traits qualitative and others quantitative. Krout and Burtt think that most traits are known by their opposites rather than by degrees of difference. Marsh and Perrin deny the quantitative distribution of traits. Triplett and Anderson maintain that there are qualitative differences between children and adults. McCarthy, Pearson, and Jersild refute this viewpoint.

On the basis of the foregoing facts, it appears that each succeeding concept facilitates man's control of the social world. New experiences make inadequate the control provided by preceding terms. Succeeding concepts organize experiences into answers to inherited old problems, as well as entirely new problems. Personality trait is integrally related to several new notions, *viz.*, personality, self, experience, etc. Consequently, it articulates new meanings that could

not be readily attached to antecedent concepts that were functionally related to earlier collective thoughts. Furthermore, personality trait expresses an almost unique creation of the terms, personality and trait. Earlier similar concepts such as soul-faculty and character-trait did not embrace the emphases upon self, experience, interaction, motive, integration, etc., that are present in personality trait. These unique features of the present concept are usually recognized by contemporary writers.

Less obvious are the common features of the successive concepts. This article has been primarily concerned with these. A scrutiny of the problems of earlier concepts (besides the four considered, others are the physiological localization of human attributes; vocational placement; classification of human qualities; measurement devices; and types of persons), the manipulation of them, and the answers to them, can be an invaluable guide to the contemporary research worker in the field of personality traits. It will help him in the avoidance of past errors, in a fuller realization of recurrent difficulties in dealing with similar problems, in a more perspicacious recognition of suggestive leads that might bear fruitful results, in a growing awareness of the relative position and probable course of the concept of personality trait. In common with other concepts, personality trait refers to the past, and in its present utility suggests the future activity of concepts that will supplant it. The vogue of new concepts is seldom without roots in the problems of antecedent concepts.

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